

Psychological Review

THEODORE M. NEWCOMB, Editor
UNIVERSITY OF MICHIGAN

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ROBERT MEARNS YERKES

THE PSYCHOLOGICAL REVIEW

ROBERT MEARNS YERKES

1876-1956

Robert Mearns Yerkes was born in Breadysville, Bucks County, Pennsylvania, not far from Philadelphia, on May 26, 1876. He died on February 3, 1956, in New Haven, Connecticut. In his death, both psychology and biology lost an outstanding research scientist and organizational leader.

As pictured in his autobiography (20), his childhood was the active one of a boy on a farm. He liked to work with horses and cows. He had many small, wild-animal pets. He collected tortoise and snake eggs, and watched them hatch with keen interest. His uncle, a physician in nearby Collegeville, provided him with work, so that it was possible for him to earn his way through Ursinus College. On graduation, he had intended to study medicine, but an unexpected offer of a loan of \$1,000 for graduate work at Harvard led him to go to Cambridge. At this institution he had the good fortune to study with, and to come to know intimately, most of the philosophers, psychologists, and biologists of the Harvard of that great day. He was a student and special protégé of Josiah Royce. He was also associated with George Herbert Palmer, Francis Peabody, George Santayana, and Ralph Barton Perry among the philosophers. His teachers and associates, among psychologists, included William James, Hugo Münsterberg, Robert MacDougall, and Edwin B. Holt. In the laboratory of zoology, he worked with E. L. Mark, G. H.

Parker, C. B. Davenport, and W. E. Castle. His thesis at Harvard dealt with the sensory reactions and the physiology of the central nervous system of a type of jellyfish.

As student and teacher he stayed at Harvard from 1897 to 1917. He speaks of this period as made up of happy, eventful years of research and teaching. During this time, he created and developed the study of comparative psychology at Harvard, and he also found time for many other important scientific activities. On a brief leave of absence, he acquired knowledge of neurosurgical techniques by working at Johns Hopkins University with the great brain surgeon, Harvey Cushing. Later, he worked with one of his former students, G. V. Hamilton, a medical investigator who, in his private laboratory in California, had the important new idea of working with monkeys to illuminate problems of human behavior and human psychopathology.

Under the inspiration of Dr. Ernest E. Southard, Professor of Neuropathology in the Harvard Medical School, Yerkes worked on a number of problems in human neurology and psychiatry. For five years he gave half his time to the direction of psychological service and research in the Psychopathic Department of the Boston State Hospital, in association with the remarkable group of young students who clustered about Dr. Southard at this time. It was during this period that the Yerkes

Point Scale for Measuring Mental Ability was developed. (14). In 1913, with a former student, D. W. LaRue, he published his *Outline of a Study of the Self* (11). His interest in the relationship of family traits to the personality, as treated in this book, is also attested by the fact that he sent to the National Academy of Sciences, for the file it maintains on each member, a Eugenics Record Office blank completely filled out concerning himself and his near family. In this blank, under the heading "Special tastes, gifts, peculiarities of mind or body, character, favorite pursuits, amusements, etc.," he wrote of himself (in 1912), "Diligent student from youth. Love for research. Left-handed. Poor mechanical memory."

In his fortieth year, in the spring of 1917, he accepted a call to leave Harvard and go to the University of Minnesota to reorganize the psychological work there and take direction of its laboratory of psychology. Among those who were brought to Minnesota by him at this time were Richard M. Elliott, William S. Foster, Mabel Fernald, and Karl S. Lashley. Herbert Woodrow was already at Minnesota at that time. The declaration of war, however, made it seem inappropriate for Yerkes himself to go to Minnesota and, as it turned out, he never physically took up residence there.

In this crisis year he was President of the American Psychological Association. Because of the fortunate fact—for psychology and for America—that he held this office at this time, and because of his energy and his rare gift for the effective and rational planning of organized scientific programs, he saw the challenge that the war could give to psychology and he proceeded to take quick and positive action. More than anyone else, he was responsible for mobilizing almost all of American psychology of the time and placing it at

the service of the nation. With a group of colleagues, he organized and became head of the psychological work and examining in the new and rapidly expanding Army. At first, he was given the rank of major and later promoted to lieutenant colonel. Under his direction, 115 officers and more than 300 trained enlisted men gave intelligence tests to 1,726,966 individuals. Of this number 42,000 were commissioned officers. The work accomplished by this professional psychological group is described in full in the great report, "Psychological Examining in the United States Army," which he edited and which was published by the National Academy of Sciences in 1921 (18).

During this same period Yerkes served as chairman of a committee of psychologists of the newly organized National Research Council. He helped to transform this committee into its present and lastingly effective form as the Division of Anthropology and Psychology of the National Research Council.

Because of his war work, Yerkes saw, possibly more clearly than any other scholar of his generation, the real place and importance of psychology in the service of a complex modern nation. At the end of hostilities, therefore, instead of going to Minnesota, as he was anxious to do, he felt it his duty to stay on in Washington to help in the organization of science in general and of psychology in particular in a nation at peace.

In achieving this objective, he was active in the development of the notable Research Information Service of the National Research Council. This office provided, among other services, the following: a catalogue of the research personnel of the country, a list of research laboratories in industrial establishments, a current list of scientific investigations, an index of published bibliographies of science, a catalogue of major items of scientific apparatus, a list of doctorates

in science in American universities, and many similar services. Owing to a series of misfortunes too complicated to describe here, but related to a postwar national need for financial retrenchment and a failure in certain quarters to achieve a modern vision of the new and truly national place of science in a complex society, this work was eventually terminated. Not until the Second World War, in some functions of the Office of Scientific Research and Development, in the National Roster of Scientific and Specialized Personnel, and later in the activities of the National Science Foundation and the Bio-Sciences Information Exchange of the Smithsonian Institution, were some of these early and constructive administrative ideas again brought into being. The analytic "codes" used by Yerkes in classifying research workers in these early studies were a great step forward in the whole history of the classification of fields of science, as well as of scientific workers.

During his Washington period, under the inspiration of E. W. Scripps of the Scripps-Howard newspapers, and through the instrumentality of Dr. W. E. Ritter, Yerkes participated in the organization of Science Service, the non-profit institution for the proper popularization of science, which still does much to make American newspaper and periodical reporting of science accurate and effective.

During these same active years, he was concerned in the development of two important committees which were organized at the National Research Council. One of these was the Committee on Scientific Problems of Human Migration which was organized under Yerkes' chairmanship in 1922. It grew out of correspondence between Yerkes and certain members of Congress concerning problems of immigration. The committee considered, from the point of

view of natural science, the complex migrational situation resulting from the World War. It also prepared and undertook the coordination of research programs intended to provide reliable information on the physical, mental, and social characteristics of ethnic groups. Another committee was that for Research in Problems of Sex. At the first meeting of this committee, in 1922, Yerkes was elected chairman. He held this post for 25 years. During this time the committee expended \$1,456,745, largely in grants for scientific research. It is recorded that 585 individuals participated in this work.¹

In 1924, after this amazingly active and fruitful period in Washington, and seven years after leaving Harvard, he accepted a professorship in the Institute of Psychology at Yale, the predecessor of the Institute of Human Relations at that University. The great development of these institutes was encouraged by the distinguished psychologist, James R. Angell, who was then president of Yale.

A study of Yerkes' publications, from his first paper on the light reaction of certain crustaceans in 1899 (1), shows in a most dramatic way the depth and also the amazing breadth of his scientific and administrative accomplishments.² Many of these papers deal with the sensory and neural processes of a wide variety of living organisms. Special attention is given to the speed of reaction, to inborn behavior, and to the learning process in these publications. Among the most important of these early contributions are those

¹ S. D. Aberle, & G. W. Corner. *Twenty-five years of sex research: history of the National Research Council Committee for Research in Problems of Sex, 1922-1947*. Philadelphia: Saunders, 1953.

² *Psychological Register*, Vol. 3. Worcester: Clark Univer. Press, 1932; *Publications from the Yerkes Laboratories of Primate Biology, Inc.*, Orange Park, Fla. (rev. to 1953).

which deal with habit formation in the crab (2), the sense of hearing in the frog (3), and objective nomenclature in comparative psychology and animal behavior (4). A book, *The Dancing Mouse* (5), and a paper with S. Morgulis on the method of Pavlov (6), were possibly especially significant. This latter paper, published in 1909, was most important in introducing the idea of the conditioned reflex to English-speaking readers.

In 1911, he published, with John B. Watson, the often-quoted monograph on methods of studying vision in animals (7). In the following year, his well-known paper on the intelligence of earthworms appeared. In 1913 his work on the heredity of savageness and wildness in rats did much to start a whole tradition of research in psychology (12). About this time, also, he published a paper on color vision in birds (15).

Interspersed with these technical and psychobiological studies are papers in what may be called quantitative applied psychology. This illustrates a fact not always recognized in psychology, that the thoroughly trained comparative psychologist has many techniques at his command which are especially useful in psychotechnology, or what is now called human engineering. In Yerkes' case this tendency is illustrated by his study of the psychological aspects of illuminating engineering (8), and in his experiments on the psychology of advertising materials (10).

In his Harvard period he wrote on the relation of psychology to medicine (13), and indeed much of his time there was given to this topic, as illustrated in his development of new approaches to the measurement of mental capacity, and in his studies of mental examinations of police court cases (16). He published a textbook, *Introduction to Psychology* (9), in 1911. In 1916 his notable

monograph on the mental life of monkeys and apes was issued (17).

After he went to Yale in 1924, his papers began to show a special preoccupation with the broad and significant problems of psychology, biology, and medicine, which he saw as related to the life of the chimpanzee and the other great apes. Possibly the most notable publication of his life is *The Great Apes: A Study of Anthropoid Life* (19). In reference to this work he writes in his autobiography: "In 1905, when I was fairly started in my career as a psychobiologist, I began a partnership with Ada Watterson (Yerkes), which perfectly blended our lives and incalculably increased our professional and social usefulness. Successful marriages appear in these times to be not unworthy of record and remark. Moreover, from 1905 my professional autobiography is no longer mine alone. At this moment our partnership is publishing jointly, as the outcome of six years of continuous preparatory labor, a book on anthropoid life, *The Great Apes*" (20, p. 391).

In the months between the close of his administrative work at the National Research Council and his assumption of duties at Yale, he devoted the summer of 1924 to research in anthropoid behavior in Havana in the primate colony established there by Mrs. Rosalia Abreu.

Once located in New Haven, Yerkes threw himself with energy into the establishment of a special laboratory for the full psychobiological study of the great apes. The whole growth of this idea is set forth in his book, *Chimpanzees: A Laboratory Colony* (21), published in 1943, which describes the history of the scientific study of the chimpanzee and also the development of the laboratories which were essentially Yerkes' own creations. In the epilogue of this volume, he notes that in 1900, while he was still a graduate

student at Harvard, the idea of a special research institute for comparative psychobiology suddenly came to him. From that time on, in spite of war and administrative duties, he worked earnestly to bring into being something which, in its completeness, was altogether new in the world. This novel conception was an adequate research institute for the comparative study of behavior, with special emphasis on the investigation of problems related to the great apes.

In 1925, with the cooperative support of Yale and the Rockefeller Foundation, he was able to begin to test out, in fact, the feasibility of keeping and using chimpanzees in New Haven for experimental purposes. At length, further funds became available for the realization of his dream of the establishment of a laboratory in a region more favorable for the life of these great animal subjects than was New England. After detailed study, a sub-tropical site at Orange Park, Florida, was purchased. This location, about 15 miles from the city of Jacksonville and near the St. Johns River, has proved to be most fortunate. In 1930 the erection of basic buildings began here, and the nucleus of the chimpanzee colony was established. When Dr. Yerkes retired as Director of these laboratories in 1941, it could be said with assurance that he had demonstrated unequivocally the great scientific importance of this unique primate laboratory in its sub-tropical climate. Yerkes described the success of this work under the following five headings (21): (a) The laboratory had shown itself to be capable of successful breeding and rearing of experimental animals of known ancestry and history. (b) Practical problems related to feeding, housing, and hygiene involving these large, intelligent, but very destructive animals, had been worked out. (c) Much basic information con-

cerning the anatomy, physiology, and psychology of the individuals of the colony had been acquired. (d) A system of laboratory and colony records had been established which provided a biography of each animal. (e) Special studies had been carried out dealing with growth, maturation, sexual cycle, auditory, visual, and perceptual processes, known correlates of behavior, discriminative learning, development of the use of tools; study of symbolism, ideation, and insight; a study of linguistic expression and capacity, of emotional traits, social behavior, drug addiction, and many other problems.

After Dr. Yerkes' retirement in 1942, the name of the Orange Park laboratories was appropriately changed, in his honor, from the Yale Laboratories of Primate Biology to the Yerkes Laboratories of Primate Biology, Inc.

This is not the place to evaluate in detail the many important contributions made at the Yerkes Laboratories. A list of the workers who have been trained at this station, and a study of upwards of 400 research papers that have been issued there, attest the significance that this institution has had, not only in furthering modern scientific study of the higher apes, but also in the study of the neural and general physiological basis of behavior. At the time of his death Dr. Yerkes was still an active member of the Board of Scientific Directors of the Laboratories. Those who have served with him on this Board can all attest to the wise counsel that he had given to the Laboratories since his retirement as Director. This active support meant much to Dr. Karl S. Lashley, who succeeded him as Director. At Lashley's retirement in 1954, Dr. Yerkes was pleased by the selection of Dr. Henry W. Nissen as Director. It is a cause for real satisfaction that he lived to see the distinguished work done at the Laboratories under Lashley's

direction and the fine scientific programs now being conducted there.

During the Second World War, Yerkes was active in consulting services in the War Department and at the National Research Council. For him, the second war brought again into a new focus some of the old problems with which he had wrestled so successfully during the First World War. One of the committees of the Division of Anthropology and Psychology of the National Research Council during the Second World War was the Emergency Committee in Psychology. Dr. Yerkes served as chairman of one of its most important and fruitful subcommittees—that on survey and planning. Out of the work of this group grew the notable Constitutional Convention at which the present divisional structure of the American Psychological Association was formulated. Yerkes served as temporary chairman of the convention. The constructive work of this meeting in establishing an organization which held American psychology together as a single body, instead of allowing it to dismember itself into separate organizations, was in no small measure the result of his wisdom and long-tested administrative skill.

Dr. Yerkes was elected to membership in the National Academy of Sciences in 1923, and to the American Philosophical Society in 1936. As already noted, he was president of the American Psychological Association in 1916–17. He was president of the American Society of Naturalists in 1938. Among other organizations in which he was active are the American Academy of Arts and Sciences, the American Physiological Society, and the Society of Mammalogists. He was a member of the Cosmos Club of Washington, D. C. He received honorary degrees from Ursinus, Wesleyan, and Yale, and in 1954 he received the New York Zoological Society's gold medal for his pioneer work in psychobiology.

He is survived by his widow, the former Ada Watterson; a son, David Norton Yerkes of Washington, D. C.; a daughter, Roberta Watterson Yerkes of New Haven; and a brother, William Augustus Yerkes of Southampton, Pennsylvania.

Dr. Yerkes never retired. Up to the very time of his death, he was actively concerned in thinking about problems of science in relation to comparative psychology and the improvement and survival of *Homo sapiens*. In 1945 he spoke on chimpanzees as servants of science, on one of the notable nationwide series of radio talks of the New York Philharmonic program (22). In 1946 he published a thoughtful, forward-looking paper on psychology in world reconstruction (23), and in 1951 a "gorilla census" (24) which focused attention on the need for more study of that primate. His last publication is entitled "The Biologist's Point of View" (25). Certainly the title of this paper may almost be taken as the motto of his life and work. In everything that he did in the study of lower organisms or of the higher apes, as well as in his dealing with human beings in his many and important administrative associations, he always thought and acted as a humane natural scientist dealing with complex independent units of organized protoplasm.

One sentence from this last paper well summarizes the guiding faith of this distinguished scientist and great man: "Even though relatively few of us may be scientists occupationally, everyone can and should be scientific in spirit and understanding" (25).

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THE APPLICATION OF DIMENSIONAL ANALYSIS TO LEARNING THEORY

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Dimensional analysis is an analytical method by means of which one can obtain functional relations between variables in a physical problem. The variables must be capable of being reduced to an expression in terms of a fundamental set (in mechanics, mass, length, and time). This paper has the twofold purpose of (a) proposing a fundamental set for some psychological variables, and (b) applying the method of dimensional analysis to these variables.

This mode of analysis rests on the premise that an equation expressing the relationship between a number of variables has meaning only if these variables have the same dimension, where a dimension is any product of the fundamental set.² (In mechanics this would be $M^aL^bT^c$, where a , b , and c are positive or negative integers or zero.) In simple terms, this statement asserts that one cannot add apples and bananas unless one "reduces" both to fruit. An equation in which the variables have the same dimension is called *dimensionally homogeneous*. As this concept is fundamental to dimensional analysis, it will be expounded in some detail.³

The term homogeneous as applied to algebraic equations implies that each term in the equation has a com-

mon factor, say, x . This factor can be divided out and the equation will still hold true, e.g., $5x - 4xy = 0$. This equation is said to be homogeneous in x .⁴ Equations involving dimensions, where a dimension is the qualitative characteristic of an object, are no exception. (Object is used here as a member of a class containing at least two terms which can be characterized both quantitatively and qualitatively.) Consequently, x could be a dimension and then the equation would be said to be homogeneous in the dimension x , or simply dimensionally homogeneous. If y were considered to be a dimension, the equation would of course be meaningless. In a dimensionally homogeneous equation we can cancel the dimensions and are left with a relation that involves only pure numbers, and is thus independent of the method of measurement that was employed in establishing the equation in the first place. Since the laws of arithmetic apply only to relations of pure numbers, it is clear that in order to manipulate our equations we have to make sure that we handle equations that are dimensionally homogeneous. This requirement is the foundation on which we will build our analysis.

It is now necessary to demonstrate that dimensional homogeneity is a requirement to be fulfilled by all functions containing psychological variables.

⁴ There exist more rigorous definitions of homogeneity for equations of higher degree, which, however, are unnecessary for this paper.

¹ In the Department of Aeronautical Engineering.

² This is a purely mathematical definition of dimension. An equivalent definition is: A dimension is the qualitative aspect of a quantity.

³ A detailed account of the method and its uses can be found in Bridgman (3) and Langhaar (5), both of which require a minimal amount of mathematical background.

The relationships between two psychological variables can be essentially of two natures. It can be (a) in the form of an equation expressing the functional relation between variables, or (b) of a correlational type. In the first case, one can equate two classes of objects only if both exist and both contain *equivalent elements* (6, pp. 183-185). As an object is characterized by a quantity and a dimension, an equation must by definition be dimensionally homogeneous. In the second case, it could be argued that one could fit a curve to experimental points and consider the curve as describing the relationship of the two variables, maintaining that nothing in the operations performed actually demanded dimensional homogeneity. Consider the simple case where the relation was found to be approximately linear. The curve fitted to such points would be of the form: $y = a + bx$, and let us assume furthermore that y and x have different dimensions but have been demonstrated to be related to each other. However, b is nothing but dy/dx and $a = y_0$, i.e., the value of y at $x = 0$. Rewriting the equation as $y = y_0 + (dy/dx)x$, we immediately recognize it as a dimensionally homogeneous one. In case the relationship is not linear, but is "well-behaved"⁵ (a most plausible assumption to make) the function can be expressed as a Taylor Series, which, by the very mode of its construction, is dimensionally homogeneous.⁶

⁵A "well-behaved" function is one which changes smoothly and continuously. This does not exclude oscillatory and similar functions.

⁶Any "well-behaved" function of an arbitrary number of variables can be expressed as a Taylor Series. As a matter of fact, the mathematical definition of well-behavedness is equivalent to the statement that the functions can be expanded as a Taylor Series. Every standard treatise on the calculus can be consulted on details about the Taylor Series.

One is thus forced to conclude that the functions of psychology should also fulfill the requirements of dimensional homogeneity. There are two implicit assumptions involved in this presentation. The first is that the variables are measurable by at least an interval scale. (The scaling method that dimensional analysis generates will be shown later.)⁷ The second assumption is that deterministic laws can be found in the particular area of investigation. The possibility of describing the phenomena probabilistically is not rejected. As to which of the two approaches is the more fruitful one, only time will tell, and in the meantime we find comfort in a statement by Einstein in a letter to Max Born. "You believe in the dice-playing God, and I in the perfect rule of law. . . . I am absolutely convinced that one will eventually arrive at a theory in which the objects connected are not probabilities . . ." (2).

The necessity for dimensional homogeneity imposes certain restrictions on the relationship in which variables can appear in an equation. Thus by knowing the variables but nothing about their relationship to each other, one can by some very simple manipulations find their functional relationship.⁸ Upon considering the applicability of this method to psychology, one is confronted by the problem that no independent members of a fundamental set, or basic dimensions, have been ascribed to psychological variables. The place of dimension in

⁷Coombs has developed the notion of four types of models—the compensatory, conjunctive, disjunctive, and lexicographic. Dimensional analysis is a method usable only on compensatory models. Thus, if the method does not turn out to be useful, the possibility that the variables under consideration do not act in a compensatory manner is always possible.

⁸See footnote 3.

physics can be illustrated by the following quotation:

In the domain of mechanics we are called upon to deal with various aspects of nature to which we give such names as length, mass, time, force, velocity, momentum, energy, etc. *By common consent* (our italics) we take the first three of these as basic and independent. . . . If we then fix units for the measurement of these three basic quantities, we are immediately in a position to express the measure of any other quantity . . . in terms of these basic units (5).

We now have to proceed by analogy from the physical sciences and assume the existence of a fundamental set of variables (which are measurable *directly* or *indirectly* with a certain degree of precision) such that all other variables in the particular area of investigation can be expressed in terms of this fundamental set. A purely hypothetical example will illustrate this point; if tension and attention are considered basic dimensions, then perceptual vigilance or defense could be stated as changes in attention with respect to tension.

Dimensional analysis can thus be used for the following purposes:

1. Given the variables that are relevant in a particular situation, and given the dimensions of these variables, a function can be found which expresses the relations of all these variables to each other *without* experimentation, only by means of some simple analysis.

2. In case the variables known to be important in a particular situation cannot be combined into a dimensionally homogeneous equation, it can be construed as a sign that an important variable is missing—or certain variables could be neglected. As the dimensions of the supposedly missing variables may suggest themselves upon examination, this has an important heuristic function. A simple example from mechanics will be introduced so that we will not have to deal in ab-

stract generalities. It was known from experimental investigations that

$$R \propto S/a,$$

R being the resistance caused by two bodies sliding over each other, say submersed in oil, which is proportional to the relative speed S , and the distance of separation a . The dimensions of these variables are $R = F/L^2$, $S = L/T$ and $a = L$. This equation can also be written as

$$R = K \times S/a,$$

where K is called the constant of proportionality, whose dimension must be chosen so as to make the equation dimensionally homogeneous. It will obviously have to be TF/L^2 . This variable was called the coefficient of viscosity, and was found later to be an important variable in physical problems. In addition, the dimensions of this variable can be fully justified on the basis of fundamental laws of mechanics and make good *physical* sense.

3. Dimensional analysis can be used to scale psychological variables. The discussion of this application will be postponed to a later point in the paper until a specific example from the psychology of learning can be utilized.

Dimensional analysis can *not* tell the investigator which variables are functioning in a situation (except in the sense of point 2, above). It is *not* a theory—in the sense of a guide for the selection and definition of variables. It is merely an analytical tool, useful once the selection of variables and the assigning of dimensions to these variables has been accomplished.

The important thing to remember is that there is absolutely nothing about dimensions that is God-given or intuitively apparent, apart from the fact that dimensions are variables in terms of which all other variables can be expressed.

Application to Hull's Variables

Now that the method has been explicated the afore-mentioned purpose of the paper will be attempted. It is fully realized that the dimensions arrived at are only tentative, and might turn out to be quite fruitless, though this would not necessarily imply the lack of utility of the approach.

The variables in the following equation will be expressed in terms of *drive* and *time*, which will be considered as basic dimensions.

$$s\bar{E}_R = f(sH_R, V, J, K, D, sI_R, I_R) \quad (4).$$

Reinforcement itself is not included as a variable but, as it plays an important role in the other variables, dimensions will be assigned to it. As drive reduction occurs in an interval of time, reinforcement can be thought of as a change of drive with respect to time, i.e., it is expressible as a derivative $\frac{dD}{dT}$ or DT^{-1} .

K is the incentive, and its amount is assumed to determine the slope of the reinforcement curve; it can therefore be expressed as the second derivative $\frac{d^2D}{dT^2}$ or DT^{-2} .

V represents the stimuli evoking the response. It was found very difficult to define their dimension in terms of their function, but as Hull assumes them to attain secondary reinforcing value, they will, provisionally and with misgivings, be considered as having the same dimension as reinforcement.⁹

sH_R is a function of the number of reinforcements, and its dimension is therefore the same as that of reinforcement itself.

J , the delay, has an effect that is opposite to K . It can also be thought of as determining the slope of the reinforcement curve, and its dimension is therefore the same as that of K .

⁹ See footnote 3.

I_R and sI_R can be thought of as making for an increase in drive which lessens the strength of sE_R . Their dimension is therefore equivalent to reinforcement, though opposite in its effect.

sE_R will be considered to have the dimension of time because it is measurable directly or indirectly in terms of latency scores.

To summarize the dimensions of the variables:

$$sE_R = T \quad K = DT^{-2}$$

$$sH_R = DT^{-1} \quad J = DT^{-2}$$

$$sI_R, I_R = DT^{-1} \quad D = D$$

These variables may now be grouped into nondimensional clusters, a nondimensional cluster being a group of variables which is a pure number, e.g., $\frac{sH_R}{sI_R}$ or dimensionally $\frac{DT^{-1}}{DT^{-1}}$.

Some of these groupings might turn out to be important in experimental situations, i.e., the variables function interdependently—a change in one with a concomitant change in the other (so that the value of the nondimensional cluster remains constant) should result in no change of an independent variable.

Two such nondimensional ratios may be plotted against each other by systematically varying only one vari-

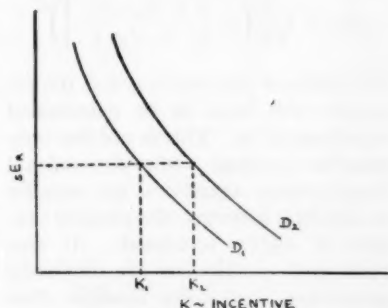


FIG. 1. Illustration of scaling technique.

able in one and obtaining measures for only one variable in the other, and thus achieving a two-dimensional graphical representation of more than two variables.

Now the use of dimensional analysis in scaling can be shown. If a cluster has been demonstrated to be truly nondimensional, then it can be plotted as shown in Fig. 1. If D_1 is six hours of deprivation and D_2 is twelve hours of deprivation, and we assume the one to be twice the amount of the other—i.e., we use these as a base line—then K_1 can be considered to be twice the amount of K_2 . Or, more cautiously, the relationship of the magnitude of K_1 to K_2 is the same as that of D_1 to D_2 .

With the above-mentioned variables the following nondimensional clusters were formed:

$$\frac{KD}{J^2 \times (sE_R)^2}; \frac{sH_R}{sI_R}; \frac{K}{J}; \frac{sH_R \times I_R}{V \times sI_R}$$

Whether these are truly nondimensional will have to be determined experimentally.

Assuming these clusters to be nondimensional, the following combination into an equation is possible:

$$\frac{sE_R^2 \cdot K}{D} = C_a \left(\frac{K \cdot I_R \cdot sH_R}{J \cdot sI_R \cdot V} \right)^b$$

$$sE_R = \sqrt{\frac{D}{K}} \left(C_a \left[\frac{K \cdot I_R \cdot sH_R}{J \cdot sI_R \cdot V} \right] \right)^{\frac{b}{2}}$$

The value of the constant and the exponent will have to be determined experimentally. This is not the only possible equation. Other dimensional homogeneous equations are equally acceptable; however, the number possible is highly restricted. At this point such predictions are obviously premature, but quite feasible after some systematic investigation.

Preliminary Experimental Investigation¹⁰

Some evidence that the present analysis might be adequate was gained by the following pilot study.

The purpose of the experiment was to establish whether the variables K and D were compensatory as the above analysis would suggest. (See the nondimensional cluster

$$\frac{K \times D}{J^2 [\text{Const.} = sE_R^2]})$$

Procedure. It was first necessary to establish whether D and K (of which preference was considered to be the index) were independent of each other. Three different saccharine solutions were presented to two groups of albino rats ($N = 24$) under conditions of 24 and 8 hours of water deprivation. They had free access to all three solutions, and the amount consumed of each was recorded. Both groups preferred the same solution, namely, that found optimally preferred by Beebe-Center (1). It was therefore concluded that D and K were independent.

In the experiment proper, two groups of albino rats were trained in a Skinner box. For each bar-pressing a drop of fluid was released into a plate. The animals were trained until their average pressing rate was constant over a 3-day period. One group was trained under 6-hour water deprivation and presented with the optimally preferred saccharine solution, the other being trained under 22 hours' deprivation and reinforced with plain water.

The sE_R measure taken was rate of response after it had leveled off. The mean response for the two groups was

¹⁰ The authors would like to express their sincere thanks to Dr. Russell Clark who guided the design and execution of these tests.

TABLE 1
RATE OF BAR-PRESSING IN SKINNER BOX
UNDER TWO EXPERIMENTAL CONDITIONS

	Group I	Group II
	8 hrs. deprivation saccharine solution reinforcement	16 hrs. deprivation water reinforcement
Mean	4.08	4.06
SD	.45	.38

calculated, the results being as shown in Table I.¹¹

Conclusion. The difference between the two groups would occur by chance 100% of the time as $t = 0$. It was therefore considered reasonable to *accept* the null hypothesis.

SUMMARY

The method of dimensional analysis was presented. Its uses as a method of combining variables into functional

¹¹ A more detailed report of experimental procedure and findings can be obtained upon request.

relationships by analytical considerations, as a way of discovering new variables, and as a tool for scaling were discussed. Some variables of the psychology of learning were dimensionalized and analyzed by means of the above methods. These considerations yielded a number of predictions, one of which was subjected to a preliminary test. The results were as predicted.

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A LANGUAGE FOR PERCEPTUAL ANALYSIS

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The aim of this paper is to develop further the theory of behavior that has already been outlined by the authors in a previous publication (3).² In particular, it aims to develop that part of the total theory which deals with *perception* and to clarify what has sometimes been called the "nativist-empiricist controversy." The method we have adopted in presenting the over-all theory is to proceed by degrees toward a state of greater precision by selecting for elaboration at each stage some single aspect of the whole theory. It is at present a skeleton theory, where we are attempting to lay down a *language* to deal with the principal problems of behavior. The aim is as much meta-theoretical as theoretical, and involves the clarification of existing terminology as well as the laying down, albeit tentatively, of rules for the *use* of such terminology.

THE MODEL: GENERAL BEHAVIORAL UNIT

The behavioral model centers around the notion of a *belief*. It is suggested that a belief is ultimately to be investigated in terms of the states of the central nervous system; but it may be thought of, in the Peircian manner, as "that which disposes the organism to

act" (8). Belief is here used as a theoretical term, a sort of logical construct.

Beliefs are considered to be relatively permanent states of the central nervous system. In everyday terms, they are those stored memories whose contents specify for the organism what may be expected to happen (S_2) if certain behavior (R_1) is performed under certain stimulating circumstances (S_1). Since at any given moment the organism's behavior is a function of a relatively few of the totality of its stored beliefs, we shall call those beliefs which are actually influencing behavior at any given instant of time *expectancies* (E ($R_1 - S_2$)), or more briefly, (E s). Beliefs may be converted into expectancies through the action of the *activating stimulus* state (S_1). This activating stimulus state is a conjunction of *motivational stimuli* (Sms), stemming from the motivational state (M) of the organism, and of the *stimulus complexes* (S^* s).

Both the Sms and the S^* s are subclasses of the class of stimuli that have been associated with particular beliefs.³ Sms are, of course, internal to the organism, while the S^* s may be either external or internal. One possible subclass of S^* s (the relation may actually be one of overlap, class exclusion, or

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² The authors wish to thank Dr. John W. Volton of the Department of Philosophy, Princeton University, for his continued helpful advice, especially with this paper.

³ We have previously defined "stimulus" as "any change of energy which impinges upon the nervous system such that it brings about activity of that system. The source of this energy change may be either external or internal to the organism" (3, p. 31). Obviously, not all such stimuli have been associated with particular beliefs.

class inclusion) is the class of *modifying motivational stimuli* (MMs) which are capable of changing the internal *motivational state* (*M*). This motivational state, which is seen as being composed of two factors, *drive* (*D*) and *urgency* (*U*), may act to determine the range of the expectancies transformed from beliefs.⁴

After a range of expectancies has been transformed from the totality of stored beliefs by the activating stimulus state (*S*₁), a hypothetical process is postulated by which the range of expectancies is *scanned*. This process of scanning leads to the "selection" of a single expectancy whose correlative response (*R*₁) is the one that will be subsequently emitted. The "selection" of the single expectancy during the scanning process is made in terms of (a) *the strength of the belief* underlying the expectancy, and (b) *the valence* of the expectancy. Valence, in turn, is a function of the *anticipated reward* and the *anticipated effort* involved in the projected response. The emission of the correlative response (*R*₁) associated with the chosen expectancy follows automatically upon the selection of that expectancy. This response will either be followed by the *anticipated outcome* (*S*₂), in which case either a *confirmation* of the correlative belief will take place, or it will not, in which case a *falsification* of the belief will follow. This whole process, beginning with the transformation of beliefs into expectancies by the *S*₁ and ending with confirmation or falsification, is called the *general behavioral unit*.

While the interactions of the above

⁴ "Motivation" has been defined by us as "a state of the organism, compounded of drive and urgency factors, which, through the medium of motivational stimuli (*Sms*), produces behavior in conjunction with stimulus complexes (*S*s*) through the elicitation, rehearsal, and selection of expectancies; and the elicitation of responses" (3, p. 33).

variables are too complex to be presented in any adequate detail here, a few further points should be made. Which beliefs are converted into expectancies depends upon the previous association between certain stimuli and certain beliefs. Beliefs are thus acquired by contiguity—the association in experience of activating stimuli (*S*₁s) with means outcomes (*R*₁ – *S*₂s). Such experiences may be direct ones, wherein physical energy changes, forming the basis for the stimuli, emanate from that event about which the organism acquires beliefs. But many beliefs are learned indirectly through the use of symbols, where the emanating energy changes from the event do not in any direct fashion determine the beliefs about that event. Thus, knowledge may be acquired either through *direct acquaintance* or through *description*.⁵ Apart from their acquisition, beliefs may be *strengthened* (through confirmation) or *weakened* (through falsification), either by description or by direct acquaintance, by going through the steps of the general behavioral unit (GBU).

Motivation is not only an important factor in the determination of the range of expectancies elicited for scanning, but it also plays an important role in the selection process through its indirect effect upon the valence. Further, motivation operates to determine the speed of selection as well as the speed and strength of response elicitation.

There are other behavioral units called the *perceptual behavioral units* (PBUs) which are closely related to GBUs, and are necessary to them. For every GBU there must exist a PBU to categorize the originally registered *activating stimulus state* (*S*₁). A PBU is also necessary to identify the *outcome* or *goal* (*S*₂) to allow assessment of *confirmation* or *falsification*. It is clear

⁵ It has been suggested that certain stimulus-belief associations may be innate as well.

that a PBU must precede every GBU and must also follow every GBU. The possible exception is in so-called "thinking," where GBUs might follow each other.

THE MODEL: PERCEPTUAL BEHAVIORAL UNIT

We shall now turn our attention to the *perceptual behavioral unit* (PBU) itself and note, first of all, the similarities and differences between it and the previously described *general behavioral unit* (GBU).

In general, the basic form of the PBU is similar to that of the GBU. After proper encoding, certain of the stimuli impinging on the central nervous system are capable of transforming beliefs into expectancies; then, through the process of scanning, one of the expectancies is selected, and a response ensues. But there are certain important differences between the details of the GBU and the PBU that must be noted.

1. The first difference concerns itself with the nature of the final response (R_1). In the PBU the R_1 is the covert response of categorizing or classifying the impinging stimuli. Such a response is best thought of as occurring entirely within the central nervous system, and as not necessarily involving conscious awareness. In contrast, the R_1 of the GBU may be any number of different actions, some of them overt (motor activity) and some covert (emotional changes, "thinking").

2. We must distinguish between the *contents* of the beliefs of the PBU and those of the GBU. In the PBU, beliefs concern themselves with such cognitive actions as seeing, hearing, tasting, etc.—in general, those activities which have traditionally come under the rubric "sensation and perception." They involve the action of the central nervous system as the organism "apprehends" its external, as well as its internal, en-

vironment. Perceptual beliefs can be expressed as conditionals of the form: if the impinging stimuli (the S_1) have been categorized as C_1 (the R_1), then the subsequent impingement by other stimuli of the categories $C_2 \dots C_n$ (the S_2) is likely to obtain, with the probability P . What the conditions are under which these probable impingements will take place also forms the content of perceptual beliefs—or, more precisely, the perceptual meta-beliefs.

For convenience's sake, we shall consider all those beliefs that are concerned with the perceptual categorization of events to be *perceptual* beliefs, and all others *general* beliefs. That such an arbitrary division as this is only a temporary verbal convenience will be appreciated when we now note the close relationship between GBUs and PBUs.

3. In the GBU, in order for an activating stimulus state (S_1) to transform a belief into an expectancy, it is first necessary that the S_1 be *perceived*. By perception, of course, we mean the action of a categorizing response which is the R_1 of an immediately preceding PBU. That is to say, in order for a GBU to take place it must be preceded by a PBU. On the other hand, in the PBU, in order for an S_1 to transform a perceptual belief into a perceptual expectancy we must arbitrarily assert that it is *not* possible that the S_1 be preceded by a categorizing response. (Indeed, if in the present theory this *were* permitted, we should obviously be involved in an infinite regress.) Thus, for the PBU, given a particular S_1 , those perceptual beliefs that are associated with the S_1 will immediately and automatically be converted into perceptual expectancies. This transformation will take place without the S_1 s being first perceptually categorized. The only categorization involved is the R_1 which ends the PBU.

4. Since there may be more than one

perceptual belief associated with a given S_1 , the question arises as to *which* of the beliefs converted into perceptual expectancies will be selected. Such a selection will lead automatically to the categorizing response, R_1 , of the PBU. We have seen that in the GBU the selection of the expectancy which leads automatically to the R_1 is a joint function of (a) the valence of the expectancies which have been transformed from beliefs, and (b) the strength of the beliefs correlative with these expectancies. In the PBU, however, while these two factors of valence and belief strength also operate to select a single expectancy, it is the latter that seems to play the more important role. This does not mean that valence may not be important, especially when the stimulating circumstance (S_1) is ambiguous and when motivation is strong; but, on the whole, the strength of the perceptual belief is primary in the selection of a particular perceptual expectancy leading to the R_1 of the PBU. By strength of perceptual belief we mean, simply, the degree or strength of association existing between a particular S_1 and a particular perceptual belief.

5. Unlike the GBU, the final step in the PBU is not an *outcome* or *goal* (S_2) which is then followed by another PBU leading to a categorizing response, for this would now involve us in an infinite progress. Rather, the PBU ends with an R_1 , the categorizing response. This perceptual categorization may, however, be subsequently confirmed or disconfirmed; and this, in turn, will lead to the strengthening or weakening of the correlative perceptual belief through confirmation or falsification. Such confirmation or falsification may take place in two ways. First, the outcome (S_2) of the response (R_1) of the subsequent GBU may either confirm or falsify the veridicality of the previous perceptual categorizing response. Secondly, a sub-

sequent PBU, because of the content of the organism's belief system, may be categorized as being either compatible or incompatible with the previous PBU in question. And this, in turn, may bring about a confirmation or falsification of the previous PBU, leading to the strengthening or weakening of the correlative perceptual belief. It should be noticed that perception is normally regarded as *certain* by the organism, and is not thought of as requiring further confirmation.

We must distinguish between beliefs regarding perceptual *events* and beliefs regarding perceptual *rules*. We might also talk of perceptual beliefs regarding *rules about rules*. Another way of distinguishing these various levels of perceptual beliefs is to call them "beliefs," "meta-beliefs," and "meta-meta beliefs," etc.

Let us now continue our examination of the PBU in detail. We have previously defined the activating stimulus state (S_1) as "... that state of the central nervous system which is capable of transforming specific beliefs into expectancies" (3, p. 32) and we have considered the S_1 to be made up of stimulus complexes (S^* s) plus stimuli arising from motivational states (Sms). But in order to specify more precisely the functioning of the S_1 in the PBU, it is convenient to partition S_1 s into three subcategories: (a) cues, (b) clues, and (c) signs.

We shall postulate, as part of the connotation of the words "cue," "clue," and "sign," that they refer to the organism's *use* of certain stimuli *after* they have been associated with specific perceptual beliefs. Thus, we shall consider cues, clues, and signs to be a subclass of the class of activating stimulus states (S_1 s) rather than, say, a subclass of stimulus complexes (S^* s) or motivational stimuli (Sms). For we wish to make it clear that cues, clues, and signs

have their functional basis not only in events external to the organism, but also in events internal to it, such as beliefs, attitudes, motivations, etc.

1. CUES (Cus): *A subclass of the class of activating stimulus states (S_1s) which stems from objects or events, either internal or external, which are being apprehended directly by the organism through knowledge by acquaintance.*⁶

Cues from external events may be modified by internal Sms stemming from concomitant motivational states. Conversely, cues from internal events may be modified by $S*s$ stemming from concomitant external events.

2. CLUES (Cls): *A subclass of activating stimulus states (S_1s) which stems from the context, ground, or surround in which the apprehended object or event, either external or internal, is imbedded. The apprehension of a clue by the organism is direct, i.e., through knowledge by acquaintance.*

Unlike cues, clues are not apprehended in and of themselves; if they are experienced at all, they are experienced in conjunction with cues, and they usually exert influence upon the apprehension of the concomitant cue, and vice versa.

Clues, then, are S_1s which inform the organism about some events or objects other than themselves. Such information may have to do with such "objective" matters as the size, color, shape, location in space, etc., of objects as well as such "subjective" matters as pleasantness, attractiveness, harmfulness, etc. Such information as is given by clues may or may not, of course, be veridical.

3. SIGNS (Sns): *A subclass of the*

⁶ See the authors' previous discussion (3) for the distinction between knowledge by description and knowledge by acquaintance. "Apprehended" is a neutral term that can be easily defined as a function of the S_1 . That is, it is simply the registering of a stimulus by the central nervous system—not necessarily in consciousness. As used in this paper, "experience" has a similar meaning.

class of activating stimulus states (S_1s) which the organism, through the acquisition of beliefs, has learned stand for other stimuli.

As can be seen by the definition, signs are closely related to clues, since they are a subclass of S_1s which are concerned with objects or events other than themselves. But, unlike clues, which must always appear with the object or event about which they yield information, signs may give information about objects or events which are not simultaneously present. In this sense, clues may be considered to be a kind of subclass of the class of signs.

As we see from the above definition, signs may come to stand for, or be substitutable for, or come to represent objects or events to the organism which are not now present. In fact, under special circumstances, they may come to stand for events or objects which can never be known directly (i.e., through knowledge by acquaintance) by the organism. Thus, though the information the clue conveys is always about an object or event known through knowledge by acquaintance, in the case of signs this need not be so. This is particularly true of signs that are encountered in language (7) which are used in formal and informal education to convey information about objects or events which may never be directly experienced by the organism.⁷ Cues as well as clues may, of course, act as signs under special circumstances.

This is by no means a sufficient analysis of signs, since that part of the glossary definition which says they "stand for" itself requires a great deal of further explanation. There is no space for this analysis here.

⁷ While it is theoretically possible for all but the very first sign-object association to be learned through knowledge by description, in most actual cases the child learns most of his early language signs by experiencing directly both the sign and the object, usually contiguously in time.

Objects and events about which cues and clues convey information can be experienced only directly, while the objects or events about which signs convey information can be experienced either directly or indirectly. Knowledge about cues, clues, and signs, however, may be acquired either through knowledge by acquaintance or knowledge by description.

The breaking down of S_1 s into the above subclasses may often turn out to be rather arbitrary; nevertheless, it is believed to be a convenient way of describing the various functions of the activating stimulus state in the PBU. The point of breaking down the integrated act of *perception*, in the way described, is to avoid the impasse of classical Gestaltism which allows too little of the necessary analytical processes of science. At the same time our reconstruction is intended to cater to the wholeness principle of Gestaltism which has been too often overlooked by classical behaviorism.

Often, when we apprehend an object or event, we are not able to categorize it completely or fully, to our satisfaction, at the first attempt. Instead, we may first categorize it one way and then another, and so on. We may, of course, take into account each one of our "abortive" categorizations to form a concatenation of interpretations from which we construct our final categorizing response. Or we may make use of further incoming information from the object or event itself. Or, finally, by examining our past experiences, through our perceptual beliefs, we may try to "remember" what this object or event "might be." We then arrive at a categorization upon which we are prepared to act, insofar as we are relatively sure that our categorization is a "correct" one.⁸

⁸ The word "categorization" is intended by us to mean both (a) the activity of separating any two parts of "sensory time-space," and

From this we see that we can look upon the perceptual process as consisting of a finite series of interpretations: I_1, I_2, \dots, I_n , where I_n is that interpretation or categorization upon which the organism is prepared to act. In the limiting case, the series of interpretations may, of course, be only two— I_1, I_2 , or even merely I_1 . The series of categorizations may take place extremely rapidly, and it is only on relatively rare occasions that such a series is slow enough and perhaps difficult enough, so that we become conscious of this process.⁹

It is therefore necessary, for any complete description of the perceptual process (or process leading to perception), to make allowances for such a series of interpretations or categorizations. We shall, therefore, distinguish between what we will term the *provisional categorizing responses* (PCRs) and the *final categorizing response* (FCR), the latter being of particular interest since it forms the S_1 of the subsequent GBU.

From what we know of visual perception, those categorizations and interpretations of "the given" (S 's and S 's) which occur early in the total percep-

(b) the subsequent activity of drawing up classifications in terms of similarities and differences. In this sense, the word has a broader meaning than is usually intended.

⁹ The reader should not infer from what has been said that we are here advocating any return to the structuralist position in which perception is compounded of elementaristic units bound together by the "cement" of association. But the rejection of such a position is not to deny the piecemeal, serial nature of so many of our perceptions. Further, when operating within a linguistic model, such as the one we are proposing, such an analytic tool as a series of terms, not necessarily functionally independent, seems absolutely necessary. Thus, there is an important relationship between the analytic (logical) and the psychological meaning of "interpretation." Any such discussion of this relationship, such as would be elaborated in pragmatics, would not be appropriate to this paper.

tual process are primarily concerned with such matters as the separation into figure-ground, form, color, texture, etc., leading to categorizations of basic recognition. Whether these categorizations and interpretations which occur early in the perceptual process are acquired through experience, as suggested by Von Senden and Hebb (10, 4), or whether they are part of the built-in functions with which we are born (6), is an issue which cannot be settled at this time. We are here merely pointing out the fact that certain perceptual processes are prior in time to others. The later categorizations and interpretations, besides correcting earlier misperceptions, tend to be concerned with such factors as the perceptual context, elaborated meanings and implications, etc. Here the earlier categorizations are combined, elaborated, and given added connotations from past experience through the action of the organism's belief system. We assume, of course, that the same process is duplicated in all the sense modalities. That such elaborations may be enormously complex can be attested to by the difficulty encountered when one attempts to specify the exact sequence of events of an aesthetic experience involved, say, in the reading of a poem or the contemplation of a painting.

As hinted at above, often it is the categorization of the context in which the object or event is imbedded that may greatly influence the FCR of that object. For example, suppose we place a straight stick in water such that half of it is submerged beneath the surface. Now, if we look at the stick from a certain angle, we may very well categorize it as being "bent" if we have not yet observed the stick to be half submerged in water, or if we are not aware of the effect that media of differing refractive indices have upon the path of light. Once, then, we are aware of the context, and have appropriate beliefs about the

influence of the context upon the object, we may categorize the stick as being "straight" even though we might continue to say that it "looks bent."

We must, therefore, at times distinguish between the way a particular object or event is *sensed* and what the object or event is *believed* to be. In terms of the present model, the earlier *sensing* of the object would correspond to the interpretations of the earlier of the PCRs, while the final *believing* of what the object or event is would correspond to the FCR. The stick in water may be *sensed* as looking bent, but it will be *believed* to be a straight stick half immersed in water.

Both *sensing* and *believing* involve the action of beliefs, expectancies, and the selection of a categorizing response, so that these two processes are really not different in kind, but are simply a matter of relative position in a series of interpretations.

We may, of course, cite other perceptual illustrations of the bent-stick type in which the PCRs are not always in agreement with the FCR. These illustrations are often used to make the point that experience may have very little effect upon what is perceived (9). Our disagreements with such an interpretation will be elaborated upon in the next section; nevertheless, these illustrations are helpful in demonstrating certain important details of the proposed model. For example, looked at from the top of the Empire State Building, cars on the street below may be said to "look like" (be *sensed* as) toy autos, but they will be "believed to be" normal-sized cars observed from a great height. Or, standing on the ties of a railroad right of way, the rails may "look like" (be *sensed* as if) they converge at the horizon, but are "believed to be" parallel. In these rather special cases, stimulus configurations which we have previously learned to categorize in

one particular way may not correspond with the actual situation in the present instance. Indeed, an important part of one's "maturing perceptually" is the learning of those contexts where it is the better part of wisdom to disregard or make exceptions to previously valid perceptual beliefs and meta-beliefs. On the other hand, *most* perceptual situations are such that the perceptual beliefs we hold do have generalizability, insofar as they have a validity which is trans-situational, and in which the PCRs do correlate highly with the FCR. In this sense, then, the examples cited above are, despite their popularity in psychological theorizing, not representative of usual perceptual situations (1).

THE SEMANTIC PROBLEM OF PERCEPTION

The analysis of the last section has some immediate applications with respect to the controversy between those who contend that experience has little or no influence upon what is perceived (the nativists) and those who contend that experience is a major determiner in what is perceived (the empiricists). This controversy can now be revealed for what it primarily is—a pseudo-problem. It is a pseudo-problem in the sense that it is not empirical; that is to say, the problem has been created by the use of certain terms in various different and ambiguous ways. Both empiricism and nativism reflect some aspects of the truth, and their differences have hinged on two different interpretations of the same set of terms. These different interpretations have led to the examination of highly selected data. Within this selection, confirmation has been found for some aspects of the respective theories. The arguments have thus taken on a certain circularity, and the result has been an increasing terminological confusion.

We have already stated the essentials

of perception for our system, and the vitally important fact that arises is that the processes which have been variously and vaguely labeled *sensing*, *seeing* (or *hearing*, *touching*, etc.), *perceiving*, *believing*, and so on, refer to the complex stages of the operation which starts (say) with the impinging of a visual stimulus on the retina, and might conventionally be said to be completed by some action leading to confirmation or falsification on the part of the organism. In our terms, it begins with S*s and MMSS, and ends with either an overt or covert response. The confusion has arisen in the past over just how these processes should be described in detail. Indeed, this is our problem: to construct a suitable terminology, or linguistic framework, for the proper discussion of these perceptual problems. This is obviously not the whole answer to the problem of perception, but it will help to avoid confusion over the description of perception and this is virtually the whole answer to most nativist-empiricist disputes.

Now we should further consider some examples that were discussed in the last section. Let us consider again a stick half immersed in water. If the observer is asked how the stick is *perceived*, he may be at a loss as to how to answer correctly. This is, of course, because the word "perceive" is vague, and may be taken to mean any subset of the set of PCRs, even including the FCR. Thus if he says he perceives the object as a bent stick, it implies that the *cues* have been adequately categorized, but not all the *clues*, and the word "perceive" is taken to apply to some subset of PCRs that have not sufficiently fulfilled their full clue function, in that they fail to tell the observer that, since the stick is immersed in water, in reality the stick is probably straight.¹⁰ By

¹⁰ It should be noticed that the clues, cues, and signs present have not fulfilled their full

"in reality," we mean in the ordinary sense of being seen under some standard conditions and with respect to (say) tactual clues. If, then, the observer took a slightly broader interpretation of "perceive," he might have included such a clue about the immersion in water, and then have said that he perceived a straight stick partially immersed in water. Of course the answer to the original question is a terminological matter, and depends on the manner in which the word "perceive" is interpreted. Exactly the same difficulties apply to the other cognitive words such as "sensing," "seeing," and "believing."

It is difficult to avoid the conclusion that nativists and empiricists have disagreed mainly over how the word "perception" should be used, and have accordingly carried out experiments which support their own particular usage. Thus, for example, Gestaltists have tended to choose abstract stimulus complexes which are almost wholly devoid of experiential factors. They have thus limited the number of clues to the minimum, while the empiricists have generally done the opposite.

Let us now consider some further examples—first, the distorted rooms of the Ames demonstrations (5). If, under the specified conditions, an observer looks at a distorted room and reports it as being normal, he may be making this statement with respect to the full range of PCRs (right up to and including the FCR); i.e., he really *believes* it is normal. He has, in the words of C. S. Peirce, "no reason to doubt" that it is normal. It may be argued that this is because he lacks the cues and clues

that tell him of the distortion. This, then, is *not* an analogous case to the stick immersed in water, where his *beliefs* may more obviously be different from his *sensings*, i.e., the clues *are* observable in the stick-in-water problem, but *not* in the case of the distorted rooms. But then it is quite apparent to any rational observer that a specially distorted room (distorted, that is, with an eye to deceive) could look normal, if viewed from a particular angle; so the reason why his interpretation (or belief) is that the room is normal is that normal-appearing rooms, in experience, *are* normal. Thus he believes this both as a result of his sensings and as a result of the rest of his relevant experience.

Now we are faced with a more difficult question. Does the observer actually *see* events differently, as a result of the experience, or does he merely *believe* these events to be different? Once again we are in terminological difficulties over how broadly or how narrowly to interpret words such as "see" in terms of PCRs. In natural language the word is vague, and there is no reason to suppose that any observer can wholly divorce what he *sees* from what he *believes*. Indeed, such evidence as we have (for example, from trained artists) strongly suggests that this is a relationship which can be greatly changed with careful training.

Thus we seem to be forced to the conclusion that there is no sensible test that can be applied to the situation where the experimenter says, "Report only what you see." This is the case both because of the vagueness over words like "see," and because of the nonverbal difficulties observers have in separating one subset of the PCRs from the total set. The most obvious answer to this psychological dilemma is to concentrate on an investigation of the sensory systems from a strictly physiological point of view. This indeed is the

function relative to some categorizing decision if the FCR is incorrectly made. That is, cues, clues, and signs have "intension" (sense) and "extension" (denotation), and all that is implied may not be extracted by the observer, although complete extraction *may* not be necessary to a correct decision.

culminating point of this analysis. It points up the near impossibility of progressing further in perceptual experiments without recourse to physiological hypotheses, and it also points out the serious difficulties attendant upon the verbal instructions in any perceptual experiment.

There are other examples, which have already been discussed in Section II, that have been quoted by writers of nativist inclinations to show the lack of need for experiential factors in perception. Let us consider the cases of "parallel railroad tracks" and "cars viewed from the top of the Empire State Building." These examples bring out the difficulties well. In each case a process of sensing (primitive recognition) takes place, and two converging lines and some tiny cars may be said to be *seen*. With respect to the parallel lines there is a clear distinction—as with the stick in water—between what is *seen* and what is *believed*. Nobody would expect (in the ordinary sense of the word) to see railway tracks as parallel from the back of a train, if by the phrase "see railway tracks as parallel" we mean "see two lines that appear to stay equidistant from each other throughout their length." One of the *meanings* (i.e., one of the strong beliefs that exist about them) of parallel lines is that they should appear to converge from certain viewpoints. Similarly with cars; one expects them to look tiny from the top of the Empire State Building. And incidentally, the philosopher's popular example is also analogous: you expect a circular coin to look roughly elliptical from certain angles.

We are certainly aware that these problems cause linguistic and logical difficulties for the philosophical theories of perception; but what problem do they offer for psychology? There is no problem. No one claims that we should see such railroad tracks as parallel, i.e., as

appearing equidistant, as part of them would from perpendicularly above.¹¹ Nor indeed has anyone argued (to the best of our knowledge) that experience would make you *see* (in this sense) such things. The whole argument is over the word "see." You may *see* (in one literal usage of the word obviously near the sensing end of the continuum) the lines as converging, and yet you may also *see* (in another sense near the believing end of the continuum) the tracks as parallel. It is absolutely clear here that the word "see" is being taken in two different senses. In this verbal confusion both nativists and empiricists must accept some guilt.

The processes we are concerned with are the series processes (the set of interpretations I_1, I_2, \dots, I_n referred to above), which we may say start with *sensing* and run through to *believing* (the successive PCRs). It is certainly clear that, in all the examples quoted, the beliefs depend, as it were, on the presence of, and the making of the correct inductions with respect to sufficient cues and clues to interpret the object and the context of the object viewed. It cannot, however, be said, even in *apparently* purely visual problems, that such beliefs necessarily depend on either visual or nonvisual clues exclusively. Nor is it always possible to separate out the *sensing* from the *believing*. For example, under artificial circumstances, such as in the use of some abstract designs used in the laboratory, or by the use of some reduction-screens in the case of the constancies, our clues are seriously curtailed, and we are forced to *believe* what we literally *sense*. Under natural circumstances where we view

¹¹ The reader should be very careful with the word "see" as we have used it here. We mean that the retinal image should not be projected onto the visual cortex as lines equidistant from each other at all points. Such a state is not possible if the lines are anywhere parallel in fact.

cars from the top of the Empire State Building, railroad tracks from the back of trains, sticks partially immersed in water, and so on, we have clues in abundance which allow us to interpret in a manner which is obviously a function of experience. The point here is, of course, that by "interpretation" we finally mean "with respect to a belief" and beliefs are built up in experience; thus in our model, the function of experience, with respect to clues, is axiomatic. However, this should not be taken to obscure the fact that experiential modifications may also be built into the sensory processes themselves, nor that certain perceptual beliefs may be innate.

The best that can be suggested at the moment is that such terms as "sensing," "seeing," "perceiving," and "believing" be used with constant awareness of their great vagueness. We shall use the terms PCR and FCR as part of what we define as the PBU, and shall generally try to avoid the use of the above terms except with special care. This implies a careful statement as to the range of *meaning* that such terms carry with respect to any particular context. For purposes of constructing scientific models, we still lack "natural" (physiologically given) criteria for the partition of the subsets of the total process. This is a specific and important problem that requires solution. In the absence of any differentiating criteria, we can only point to a particular conventional usage that we intend at any given time. Thus the process beginning with stimulation, continuing through the transformation of beliefs into expectancies, and ending with categorization, is all taken by us to be part of the process that we shall call perception. We shall thus suppose that, in our sense of the term "perception," it is indeed by very definition dependent on experience, as

well as on the limitations and vagaries of the structure of the sensory process.¹²

There is one important point that should be made clear with respect to the nativist-empiricist controversy. In distinguishing "seeing" from "sensing," to take one example, we recognize that we *can* define these words as *meaning* anything we please, but in trying to explicate them we have assumed a difference which sometimes appears to exist in ordinary usage and then suggested how such a difference could be given a sensible explication. The nativists and empiricists in their quarrel may ascribe any *meanings* they choose to their cognitive terms, but they *must* formulate their disagreements over empirical facts in the *same linguistic framework*. This we claim they have not done, nor even recognized that they have not done so. Thus if they could have agreed on, or even recognized, external questions (about their framework), they would have found there were no internal questions (2) of the kind that have appeared in their discussions.

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¹² This section has so far contained no reference to the word "sign." The discussion has been concerned with learning by acquaintance, and it will be appreciated that signs themselves are directly experienced. Indeed, clues and cues may themselves be signs; and also, signs which are neither cues nor clues occur in perceptions. This notwithstanding, it is in learning by description that signs (as the term is used by us) most frequently occur.

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AN APPROACH TO PHYSIOLOGICAL REACTIVITY IN TERMS OF A BODY-IMAGE SCHEMA¹

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PROBLEMS IN CATEGORIZING PHYSIO- LOGICAL MEASURES

It is now possible to measure with considerable precision a large number of physiological reactions in the body. One may obtain an almost endless variety of indices of physiological reaction, such as blood pressure, heart rate, finger temperature, saliva output, GSR, and respiration rate. A problem that has confronted those (1, 4, 22, 28) who are interested in such indices is to find a schema which will permit a meaningful grouping or clustering of the indices. That is to say, if one obtains ten different measures of physiological reaction from each of a group of subjects, can one functionally equate certain of the measures and not simply treat them as if they were ten disparate variables? There has been a tendency in the past to lump together the separate measures and to regard them grossly as an overall index of emotionality (3, 4). This was based on the implicit assumption that emotional response is a diffuse, unpatterned process, just a general stirring up of many physiological systems. This point of view was concerned mainly with what happens to the organism after it has been disturbed and its usual equilibrium upset. It has had relatively little to say about the pattern of physiological measures obtained from the undisturbed organism. It also has had little to say about the relationship of physiological

measures to such psychological constructs as "personality" and "traits."

A more differentiated frame of reference which has been much used in analyzing physiological reactions has revolved about the difference between sympathetic and parasympathetic response. There are obvious anatomical and chemical reactivity differences within the autonomic system that lend themselves to the sympathetic-parasympathetic distinction. In an overly simplified fashion one may conceptualize the sympathetic subsystem as functioning mainly with reference to threats to the individual, whereas the parasympathetic subsystem appears to function mainly to conserve and maintain supplies for the individual's needs and development. After Gaskell (cited in Little [25]) published his views regarding antagonistic innervation (sympathetic vs. parasympathetic) of most internal organs, there developed an increasing interest in the use of the concept to explain a wide variety of phenomena. Attempts were made to classify various illnesses as being primarily due to sympathetic or parasympathetic dominance. That is to say, it was assumed that an ideal balance between the two should exist, and that if one became dominant over the other, this imbalance would lead to organ changes which would be pathological (12). Attempts were also made to apply this imbalance concept to persons without clinical symptoms: it was assumed that one or the other subsystem might be somewhat dominant in normal individuals, and cause them to develop certain personality tendencies (36). In addition,

¹We wish to acknowledge special gratitude to Dr. Hebbel E. Hoff, Chairman, Department of Physiology, Baylor Medical College, for valuable criticisms and suggestions concerning a number of aspects of this paper.

there were efforts to take all known measures of physiological response and classify them as being indices primarily of sympathetic or of parasympathetic activity. Generally, there was little objective basis for most of the assumptions that were made regarding the relationship of the autonomic subsystems to either symptom patterns or normal personality characteristics. There was even some vagueness about what kinds of physiological measures were specifically representative of one autonomic subsystem as contrasted to the other.

More recently, Wenger (40) and others (22, 25) have approached the problem of autonomic functioning with more quantitative and controlled methods than those previously used. Wenger indicates, on the basis of his analysis of the interrelations among many physiological variables in children and in adults, that an autonomic factor score can be extracted which expresses the degree of imbalance between sympathetic and parasympathetic functioning. He has found some evidence that the direction and degree of imbalance may be correlated with degree of personality disturbance and also with a number of normal personality variables. But the degree of relationship is low and variable. It is also noteworthy that he did not find it possible to differentiate patients with stomach ulcers from normals, on the basis of autonomic imbalance scores. This is an important point, because there have been many attempts in the past (16, 35) to describe this and other psychosomatic syndromes in terms of sympathetic-parasympathetic imbalance. Wenger's inability to distinguish ulcer patients from normals in terms of autonomic imbalance has been confirmed by Little (25). One of the most interesting aspects of Wenger's work is that he has shown not only the limitations of the sympathetic-parasympathetic concept, but has also discovered

other factors which are important in accounting for the variance found in any array of physiological measures. In his factor analysis of a wide range of physiological indices obtained from normals under stress and nonstress conditions, Wenger isolated not only an autonomic factor but also other moderately well-defined factors. His results indicate that the problem of categorizing physiological reactivity is a very complicated one. Darrow (9, 10) and Cattell (4) have also found that arrays of physiological measures cannot meaningfully be accounted for in terms of one or two simple dimensions.

Malmo and Shagass (27, 28), Lacey (22, 23), Lewinsohn (24), and Wolff (43) have further complicated the problem of categorizing measures of physiological reactivity by their demonstration that subjects may display fairly consistent patterns of physiological reactivity which are unique to the individual. Their work has indicated that one individual may respond to a stress with maximal activation of a given physiological function, moderate activation of another function, and underactivation of still another function. A second individual, however, may show a hierarchy of activation which is quite the reverse in character. These patterns of response cannot be explained suitably in terms of gross concepts like "sympathetic" or "parasympathetic." Those who have noted these patterns have tried to associate them with personality constructs. It has been especially theorized that some of these patterns are correlated with the same constellations of forces that result in an individual's developing a psychosomatic symptom in one organ system rather than in another (27, 28); it has been hypothesized that just as certain kinds of psychosomatic symptoms may be one of the products of a given style of personality defense, just so may an individual's pattern of physi-

ological reactivity be significantly affected by his style of life. In both instances it is assumed that life experiences have fixated certain modes of body response, and the psychosomatic symptom is simply a more extreme example of such fixation. Several investigators (23, 25, 43) have indeed demonstrated experimentally that there is a significant tendency for individuals to manifest their maximum physiological reactivity to a variety of stimuli in those body systems that also are the focus of psychosomatic symptomatology. This whole line of thought has brought to the fore the importance of a psychological frame of reference in viewing physiological phenomena.

BODY-IMAGE CONCEPTS

The problem arises, then, as to what kinds of psychological schemas we can develop which will aid in understanding an array of physiological measures.

The schema to be suggested in this paper has grown out of a series of studies of body image that we have completed (6, 7, 13, 14). We have shown that individuals differ considerably in the attitudes they take toward their own bodies. More specifically, we have found that individuals vary in the degree to which they conceive of their body exteriors as having an armoring protective function. Some visualize their body boundaries as thick, impermeable defensive barriers. Others visualize their boundaries as thin and easily penetrated. These body-image fantasies were measured by means of two Rorschach indices which have been shown to be reliably scorable and also valid in terms of a whole series of criteria (13, 14). One of these indices, the "Barrier" score, is the sum of all responses in which there is an emphasis on the thickness, strength, or unusual covering quality of the periphery. It is illustrated by such perceptions as "cave with rocky walls," "mummy wrapped up," and "man in

armor." The second index, referred to as the "Penetration of Boundary" score, is the sum of all responses in which there is an emphasis upon the periphery as penetrated, broken, or fragile. The following are examples of such responses: "bullet penetrating flesh," "butterfly with tattered wings," "plucked chicken." In order to eliminate the effect of response total upon the two body-image scores, it is requested of subjects that they give a fixed number of responses to each blot. Details of this technique are available elsewhere (13).

Our work has demonstrated that a number of personality variables are significantly linked with the degree to which the individual emphasizes the barrier or armored quality of his body boundaries. Thus, our data (14) indicate that persons who visualize their boundaries as thick and armored manifest a higher level of aspiration, more drive toward self-expression, and more motivation for competitive advancement of the self than do persons with indefinite body-image boundaries. At still another level, we have shown that patients hospitalized with psychosomatic symptoms involving the body exterior emphasize the barrier quality of their body boundaries more often than do patients with psychomatic symptoms involving the body interior. For example, it was shown that patients with symptoms like neurodermatitis, which involves the skin, and rheumatoid arthritis, which involves the joints and striate muscle sheath of the body, greatly emphasize the barrier aspects of their body-image boundaries. However, patients with interior symptoms, like stomach ulcers or ulcerative colitis, are distinguished by the permeability that they attribute to their body boundaries. By means of suitable control groups, it was shown that such differences in body image were not merely the result of experiencing symptoms in contrasting

areas of the body. Thus, a group of individuals with skin defects due to industrial accidents was compared with the neurodermatitis group; and a group of patients with low back pain due to accidental injuries was compared with the arthritics. If the mere experiencing of symptoms in the exterior layers of the body determined the individual's body-image fantasies, one would expect the two groups with symptoms due to accidents to have as high Barrier scores as the two psychosomatic groups. Actually, it was found that each of the psychosomatic groups significantly exceeded its respective control group in Barrier responses.

Since Lacey (23), Malmo and Shagass (27), and others (43) have suggested that the same factors may be operating in the development of a psychosomatic symptom in a given organ system as are involved in the manifestation of a given hierarchy of physiological reactivity, the possibility presented itself of applying the body-interior vs. body-exterior schema to the existing literature concerning physiological reactivity. That is to say, since this body-image schema had proven meaningful relative to a variety of psychosomatic symptom phenomena, it seemed logical to examine its potentialities relative to problems of physiological reactivity. The schema is not intended to stand in opposition to other modes of classifying physiological response, but is offered for consideration as an additional dimension from which to view such response. In its application to physiological reactivity the schema assumes that although a given physiological response may be triggered by certain centers (e.g., the hypothalamus), various sectors of the body may react differentially to the excitation.² Specifically,

² It is also possible to conceive of differential exterior and interior reactivity as a function of selective central processes. That is to say, it could be assumed that there are individual dif-

ferences in the patterning of central response which result in differential stimulation of various body areas.

it is being assumed that the individual's life experiences may be one factor which results in his responding to excitation differentially so far as his body exterior and body interior are concerned. Body exterior is here defined as including the skin and striate musculature. Body interior is considered to include all of the internal viscera. This definition of body interior vs. body exterior is intended to have purely location or geographic implications, and is not at all concerned with the embryonic origins of various body areas.

The idea that differences in physiological reactivity may be linked not with conventionally defined organ systems or nervous system divisions, but rather with local characteristics of different body areas has derived support from an elaborate study carried out by Davis, Buchwald, and Frankmann (11). They investigated a whole gamut of physiological reactions in subjects under a variety of stimulus conditions, and attempted to define the patterns that stood out. The patterns of physiological response they found did not correspond to any of the usual modes of categorization in the literature. Indeed, they coined new terms to designate the observed patterns. In describing the concept they have evolved of what constitutes a pattern or system of physiological response to a stimulus they say:

... it is evident that a system in this sense does not correspond with any organ system of the body nor yet with the gross structural divisions of the nervous system. The receptors, the neural structures, and the effectors involved are each activated by a prior element in the chain, and, except for the ultimate one, activate the next succeeding element. *Local characteristics*³ of the elements will determine the character of the action, transducing it from one sort of force to another. ... One is tempted to compare the spreading to the propagation of waves in a homogeneous medium—

ferences in the patterning of central response which result in differential stimulation of various body areas.

³ Italics are ours.

a better picture would be that of a branching network, which the nervous system actually is, with the possibility of transmission at any point being able to vary somewhat independently of that at any other.

The body-image schema proposes that the local variations in "transmission" may be meaningfully considered in terms of a body-interior vs. body-exterior differentiation.

If the body-image frame of reference is physiologically meaningful, it should permit one to formulate certain ideas and concepts concerning physiological reactivity which will have some support from past observations and formulations.

DEDUCTIONS FROM BODY-IMAGE SCHEMA

Three kinds of deductions concerning patterns of physiological reactivity may be made in terms of the body-image schema:

1. It would be assumed that past observations would have uncovered reaction patterns which are linked with body exterior in distinction to body interior. Further, one would anticipate that those who had intensively studied various sorts of body reactivity phenomena would have found the body-exterior vs. body-interior differentiation to be a meaningful theoretical framework.

2. When varied physiological measures are obtained from individuals who have psychosomatic symptoms involving the body interior, these measures should indicate high reactivity of body-interior sectors. One would also expect such individuals to show low reactivity of body-exterior areas. Conversely, those with body-exterior symptoms should manifest high reactivity of the body exterior. They would probably show, in addition, low reactivity of the body interior. This second prediction is derived from the idea that those who have exterior body symptoms are indicating the unusual importance of the barrier quality

of their body exterior, and thus their relatively greater tendency to maximize reactions in this sector of the body rather than in the body interior. But those with interior symptoms are assumed to assign less importance to the body exterior and more importance to the body interior.

3. Those who manifest unusually high physiological reactivity in the exterior body layers should have a more active and aspiring attitude toward life than those who do not show high reactivity in the body exterior. This prediction is based on the finding (14) that in a normal group those who emphasize the importance of their body exterior express a relatively high level of aspiration (as defined by TAT measures).

In connection with the first hypothesis, we have searched the literature widely for conceptual models developed by others to explain their observations of physiological reaction patterns which would correspond to the schema we have postulated. There are several such models worth noting.

Burton and Edholm (2) in their recent book *Man in a Cold Environment* report a diversity of data concerning the manner in which the body adjusts to sharp temperature changes, and how it maintains long-term temperature homeostasis. In the process of schematizing the various observed temperature-regulating mechanisms they arrived at a formulation analogous to the exterior-interior concept:

The temperature of the deep tissues, the brain, the heart and abdominal viscera such as the liver is not exactly the same. . . . These are close enough to uniformity and constancy to justify the simplifying concept of a central deep "core" of the body of uniform, regulated temperature, surrounded by a "shell" of cooler peripheral tissues, whose temperature moreover is dependent on that of the environment as well as on physiological factors. Indeed, the homeothermy of the "core" is accomplished, in great measure, by the adjustment of the temperatures in the "shell."

That is, Burton and Edholm have perceived, in the very complicated physiological processes involved in the body's adjustment to the changing temperatures of the surrounding environment, a specialization in function that correlates with body exterior vs. body interior.

Kleitman (20) has described a model, having to do with levels of consciousness, which is pertinent to the exterior-interior distinction. He has developed an "evolutionary theory of consciousness" which is based on a differentiation between the effects on consciousness of stimuli originating in the viscera and stimuli coming from more exterior sources, especially the proprioceptive end organs. He points out that in the newborn infant wakefulness (consciousness) occurs briefly every four or five hours, and is related to afferent impulses from the viscera. Similarly, he says of decorticate dogs that they "almost invariably moved about for a few minutes after being fed, apparently because of afferent impulses from the colon resulting from the gastrocolic reflex. They would then defecate and immediately go to sleep" (20, p. 81). This kind of consciousness is described as "wakefulness of necessity." It is a function primarily of afferent impulses from the internal viscera. Kleitman indicates that with the myelination of the afferent system in the child, the reactivity of the cortex to sensory stimuli increases. It becomes more capable of reacting to stimuli from without, particularly to impulses originating in the proprioceptive end organs. The development of the muscular system becomes a salient source of proprioceptive excitation which, in combination with other sensory impulses, leads to a higher level of consciousness. Kleitman designates this higher level of consciousness as "wakefulness of choice." For our purposes what is most interesting about this

sharper form of consciousness is that it is strongly linked with the increasing potency of impulses originating in an important part of the outer body layers (viz., the muscular-proprioceptive system). In summary of Kleitman's views, one may say that he associates a first, lower level of consciousness with the individual's inability to react to little more than the impulses originating in the viscera of the body interior. He regards later and higher forms of consciousness as a function of increasing reactivity to sensory stimuli from without, particularly those involved in proprioception.

Lorr, Rubenstein, and Jenkins (26) factor-analyzed ratings made by psychotherapists of 184 patients whom they were treating. These ratings concerned a wide range of behavior, including body complaints. The ratings of the body complaints clustered into three different factors. One factor was labeled a "factor of endodermal dysfunction," and involves complaints relating mainly to the upper and lower portions of the endodermal tube (e.g., gastrointestinal symptoms). A second factor involves complaints of "mesodermal origin." It includes mainly symptoms which are "anergic, respiratory, and cardiovascular." The third factor is of a "cerebrotonic" order, and particularly includes skin complaints. This pattern of clusters that emerged is obviously one which conceptualizes body complaints and body symptoms as occurring in layer-like fashion, with inner layers roughly distinguished from outer layers. It too shows certain analogies to the body-interior vs. body-exterior model.

Some of the more recent advances in our knowledge of the secretions produced by the adrenal medulla also lend support to the interior-exterior concept of body response. Gellhorn (17) indicates that it is now well established that the adrenal medulla secretes not only adrenalin but also nor-adrenalin.

Funkenstein (15) reviews data indicating support for the idea that there are specifically differentiated adrenalin-secreting and nor-adrenalin-secreting cells in the adrenal medulla. He refers also to a specific innervation of these cells, and to a differentiated hypothalamic representation for the corresponding secretory nerves. Adrenalin and nor-adrenalin produce rather different effects physiologically. Nor-adrenalin tends to result in moderate increase in systolic blood pressure, more marked increase in diastolic blood pressure, and a *decrease* in cardiac activity. However, adrenalin tends to produce only mild to moderate increase in diastolic pressure, but moderate to marked *increase* in cardiac activity. Adrenalin acts more forcefully in increasing cardiac output, whereas nor-adrenalin produces some of its most marked effects in terms of increased peripheral resistance. Apropos of this point, Goldenbert (as cited in [17]) found that intravenous nor-adrenalin led to increased blood pressure mainly as the result of increased peripheral resistance, whereas intravenous adrenalin stimulated a blood-pressure rise chiefly by means of an increased cardiac output. The accumulating data concerning adrenalin and nor-adrenalin suggest that their differential physiological effects fall into a pattern congruent with a body-exterior vs. body-interior differentiation.

More recently, Funkenstein, King, and Drolette (15) have demonstrated that a predominance of either the adrenalin or nor-adrenalin response in a stressful situation is correlated with certain patterns of behavioral response. They studied 69 students exposed to a stressful frustrating situation, and classified their reactions into three primary categories. The first category, "anger-out," was directed toward the experimenter who was producing the frustration. The second category, "anger-in,"

involved self-attack and self-criticism. The third category, "anxiety," involved feelings of apprehension and fright. Significantly, it was demonstrated that the anger-out group tends to show nor-adrenalin patterns of reaction. The anger-in and anxiety groups showed adrenalin patterns of response. Such data indicate that not only do adrenalin and nor-adrenalin produce physiological effects with a differential area of body response, but also that this differentiation is meaningfully linked with a difference in overt emotional behavior that is psychologically meaningful.

As noted later in this paper, the linkage of anger-out with the nor-adrenalin response, and the linkage of anger-in and anxiety with an adrenalin response, are just what one would anticipate from the body-image schema that has been proposed. That is to say, we have shown in our previous work that individuals who emphasize the body exterior, as contrasted to those who emphasize the body interior, have a higher level of aspiration and a more active, forceful approach to situations. It is therefore noteworthy that the subjects in the study carried out by Funkenstein and King who show a nor-adrenalin pattern of response (i.e., emphasis on peripheral reaction) are those who deal with the frustration directly by expressing their anger outwardly. However, the subjects manifesting the adrenalin pattern (where the response is relatively more interior) show less ability to deal with their feelings of frustration actively, and tend to express them more in self-criticism and generalized anxiety.

Kubie (21) has been very explicit in differentiating between the organs of the body, which he calls "organs which implement our relationships to the external environment" and "organs of internal economy." Those in the first group are "innervated predominantly by the somatomuscular and somatosensory ap-

paratus." They represent that part of the individual which is most consciously directed. However, the second grouping of organs "lies deep within the interior of the body and consequently are hidden from the individual's capacity for direct knowledge of himself. Furthermore, even though some of these organs have indirect connections with the outside world, our subjective awareness of them is absent or limited." Kubie associates disorders usually referred to as "conversion hysterias" with the "external" organ system, and he associates "organ neuroses" with the "internal" organ system.

Wenger (36), in the course of a factor analysis of a variety of physiological measures obtained from adults, found a factor which he designates as "representing certain functions of the skin and peripheral blood vessels." Similarly, Sanford (32) studied a group of children and adolescents with a battery of physiological and personality measures. His analysis of the intercorrelations among the physiological measures revealed "a syndrome—consisting of flushing, sweating, skin stroking intensity, odor, acne, and palpable thyroid." That is, there was a clustering of variables having mainly to do with skin appearance and skin responsivity.

The second deduction that was made from the body-image schema involved the relationship of site of psychosomatic symptom to pattern of physiological reactivity. This deduction finds some confirmation in past work. It should be specified at this point that, in order to keep the distinction between body exterior and body interior as clear as possible, measures of exterior body functioning will be restricted to GSR, muscle action potential, and direct methods for evaluating vasoconstriction and vasodilation of the peripheral blood vessels. Measures of interior body functioning will be restricted mainly to heart per-

formance and measures of changes in the stomach and colon.

Since intestinal disturbances and stomach ulcers are interior symptoms, one would predict, in terms of the exterior-interior body-image schema, that subjects with such symptoms would show heightened reactivity relative to body-interior measures. Karush, Hiatt, and Daniels (19) investigated a variety of physiological responses manifested by six ulcerative colitis patients during psychotherapeutic interviews. They obtained measures of colon reaction, salivary flow, and peripheral vascular changes. There was clear indication that specific stressful material elicited in the course of the therapy produced correlated colonic responses in the colitis patients. But the salivary responses and the vascular bed responses (exterior) did not show meaningful correlations with the stressful therapy material. These patients with an interior symptom showed their most specific physiological responses to stress in terms of an interior reaction.

Lewinsohn (24) obtained a series of physiological measures from patients with duodenal ulcers, patients with essential hypertension, psychiatric patients in whom muscular tension was the outstanding clinical symptom, and a control group of patients with hernia, hemorrhoids, and similar difficulties. One of his clearest findings, which he could not explain, was that the absolute skin resistance of the ulcer group was significantly greater than that of the control group. This relative inactivity of the skin layer (sweat glands) in the ulcer patients corresponds to the minimizing of the body exterior which one would predict in terms of the body-image schema. It is perhaps confusing that high skin resistance is equated with inactivity of the skin layer. However, this formulation is based on the assumption that lowered resistance is a func-

tion of the degree of secreting activity of the sweat glands. Lowered skin resistance represents active response of anatomical "units" in the skin layer. Van der Valk and Groen (35) have also found that skin resistance of patients with peptic ulcer is high.

Another of Lewinsohn's (24) most clear-cut findings was that patients with muscle-tension symptoms show more finger tremor under stress conditions than the control group, and are also characterized by a significantly lower heart rate than any of the other groups. These individuals whose clinical symptom is most obviously focused in the outer layers of the body (musculature) show a high level of reactivity in the musculature (exterior) and a low level of activity in terms of an interior measure. Little (25), in a study of peptic ulcer patients, found that they were higher in dermatographia persistence than a control group. That is to say, their peripheral vessels manifested less tone (and so may be assumed to have been less active) than did those of the control subjects. It should be acknowledged that, on the contrary, Wenger (40) found that a group of asthmatics (interior symptom) has significantly lower dermatographia persistence (i.e., higher vessel tone) than normals. Little (25) also demonstrated that ulcer patients had a significantly shorter heart period than a control group. In other words, as would be predicted, they showed a relatively higher rate of response with an interior organ.

There is another interesting finding reported by Little which points up the potential explanatory value of the exterior-interior concept. Little evaluated the effects of vagotomy on the physiological reactivity of a group of ulcer patients, and compared the shifts occurring from the pre- to the postoperative period with those changes in physiological reactivity occurring in a control

group of other surgical patients from the pre- to postoperative period. He compares the two groups in this fashion:

Both the control and experimental groups showed a drop in arterial pressure after operation, yet the mechanisms that produced this drop seem to be different in the two groups. In the controls, peripheral vasodilation as indicated by increased Finger Temperature and decreased Dermographia Latency was the most relevant factor since the heart rate remained approximately constant. In the experimental group, however, there was an increased peripheral resistance, yet the blood pressure dropped in spite of it. In this group, the lowered heart rate is apparently the determining element. . . .

That is, following the cessation of the severe stress associated with the operation, both groups expressed their readjustment to the diminished stress by a drop in blood pressure. However, in the ulcer group the readjustment took place in terms of an interior organ, whereas in the control group the readjustment was more peripheral in character. This trend in the ulcer group is nicely congruent with the interior emphasis that has been postulated from the body-image schema.

The third basic deduction derived from the body-image schema focused on the idea that those who show unusually high physiological reactivity in the outer body layers will tend to be relatively high in level of aspiration, forcefulness, and drive to attain goals. Seymour (34) reports findings which are definitely in this direction. He indicates that children who show high GSR responsiveness (exterior) are significantly more interested in achievement and are more forceful in their behavior than children who are low in GSR reactivity. He further describes a study which demonstrated that adolescent boys who were perceived by their classmates as listless gave relatively low GSR responses to certain kinds of stimuli. In another phase of his data he found that

adolescent girls who were more daring and active in games gave larger GSR responses than those who were less so.

Cattell (4) describes a factor which is characterized by high responsivity in terms of one of our designated exterior measures (viz., GSR), and points out that this factor is psychologically associated with alertness, activity, and excitement. Low responsivity in this factor is correlated with sleepiness and passivity. Brown, as cited by Cattell (4), reports appreciable correlations of PGR with desire to excel. Cattell (4) describes a factor, "Nervous Disposition," which involves low PGR resistance and large PGR deflection; at the behavioral level it is linked with "alertness, hyperactivity." In the study by Sanford previously referred to (32), in which a cluster of measures prominently involving skin responsivity was discovered, it was also found that this cluster correlated positively with conscientious work and negatively with passive timidity. In a somewhat different approach to the physiological responsivity problem, Wishner (41, 42) has recently found that a physiological response cluster based on two exterior-type measures, GSR and muscle action potential, is associated with alertness. Such data tend to affirm the idea that high physiological reactivity of the body exterior is correlated with alertness, striving, and active expressiveness.

In general, it would appear that the over-all productiveness of the body-exterior vs. body-interior frame of reference has been sufficiently shown so that further exploration of the idea is justified. The next logical step, of course, is to set up a research design in which exterior and interior physiological measures are obtained from subjects who have been categorized in advance, relative to body-image indices. Such a study is being formulated and getting under way. If the schema is

supported by such research, there will then be available a frame of reference which can simultaneously embrace aspects of physiological reactivity phenomena, psychosomatic symptomatology, and personality defense reactions.

SUMMARY

We have discussed some of the problems involved in the categorization of physiological measures, and have emphasized particularly the lack of a physiological frame of reference from which to interpret patterns of physiological reactivity. In our previous work we have shown that the degree to which the individual conceives of his body boundaries as thick and armored vs. thin and permeable is significantly linked with site of psychosomatic symptomatology, and also with certain personality variables. We have suggested, on the basis of these findings, that patterns of physiological reactivity may be meaningfully viewed in terms of a contrast between body-exterior response and body-interior response. More specifically, it was postulated that those who, in their body image, emphasize the armored thickness of their body exterior would show relatively greater physiological reactivity in their exterior body layers (skin and muscle) than in their body interior. The converse of this was postulated to hold true for individuals who conceive of their body-image boundaries as permeable and easily penetrated. A range of literature has been reviewed which seems to lend support to such postulations.

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THE PHYSIOLOGICAL BASIS OF FORM PERCEPTION IN THE PERIPHERAL RETINA

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If the point of foveal fixation is held constant, and the angle subtended at the retina by that point and a stimulus object is decreased, either gradually with continuous exposure or by discrete steps with brief exposures, the appearance of the latter is perceived with progressively increasing clarity (4, 11, 12, 15, 17, 22, 30, 34, 35). There is now a large body of data concerned with qualitative and quantitative aspects of peripheral visual perception, but no attempt has yet been made to deal comprehensively with these data from a theoretical standpoint. Previous studies have been mainly of the fact-finding or exploratory variety, and have been little concerned with relating the subjective data to visual theory.

In an earlier paper (6) it was shown that the phenomena of foveal form perception, under the condition of increasing contrast between the stimulus figure and its surround, could be accounted for satisfactorily by the statistical theory of neurophysiological activity in the primary projection system of the eye. This paper suggested also that changes in nonfoveal form perception arising from changes in the retinal locus of the image might be dealt with by this theory. The aim of this contribution is to carry out a detailed analysis of these data within the framework of the statistical theory of visual perception.

No attempt will be made here to describe the nature of the statistical theory since this has already been outlined fully (6) and, together with the original statement of the theory (20), there are also comprehensive reviews of it in the psychological literature (23, 24, 31). It is sufficient to state here that this theory

employs the notion of populations of neural elements firing in a statistical fashion leading to a near-Gaussian distribution of excitation in area 17. Among the factors contributing to this distribution are reciprocally overlapping systems of fibers in the visual pathway giving rise to neural summation, neural recovery cycle characteristics, as well as diffractive properties, and nystagmatic movement of the eye. Visual acuity in its various forms is held to be a correlate of the peaking or sharpening of the cortical distribution of excitation. The sharper or more peaked is this distribution the greater is the resolution of fine detail. When the slope and height of the excitation gradient exceed certain minimum (threshold) values, the localization of an edge or contour in subjective visual space coincides with the location of maximal excitation in area 17. The clearness of a contour will depend upon the sharpness of the excitation peak and the steepness of the excitation gradient. In the case of an extended and flattened distribution, a line, for example, would appear broadened and blurred along its edges as well as tending to merge with the background.

COMMENTS ON EXPERIMENTAL METHOD

Before proceeding to an analysis of the experimental data, it is as well to examine carefully the experimental techniques employed in this type of investigation, since these can affect considerably the validity of the results obtained.

Studies of form perception in the peripheral retina have used two methods. In the *moving* method the stimulus object is moved slowly but continu-

ously toward the fixation point. An objection raised against this method (34) concerns the tendency of the image to disappear with prolonged fixation (8, 9, 33). Disappearance of the image could lead to the loss of useful introspective data relating to the appearance of the stimulus figure at various removes from the fovea. In the *stationary* method the test object is momentarily exposed for a controlled time interval at different displacements from the point of fixation. Apart from the criticism of the moving method mentioned above, there appears to be no reason why these methods should not be regarded as giving essentially similar results. It is conceivable, however, that the mode of stimulation of the retinal "on-off type" receptors is not the same under the two conditions of exposure.

An important aspect of procedure which needs to be considered concerns the knowledge of the stimulus objects which *O* possesses before undergoing a trial, or the opportunity provided during a series of trials for *O* to become familiar with the stimulus objects. Most studies (4, 11, 12, 17, 22, 30, 34, 35) have either familiarized *Os* with the stimulus figures before beginning the experiment, or arranged the trials in such a manner that *Os* would grow very familiar with these test objects during the course of a series of trials. Collier actually took steps to ensure that *Os* "were trained in every type of reaction for which data would be later recorded" (4, p. 283). Using this procedure, it would be expected that by the completion of a series of trials *Os* would tend merely to recognize rather than "see" the stimulus figures. Drury has drawn attention to this difficulty:

It is difficult, if not impossible, to estimate the effects of recognizing, instead of "seeing," the known forms. To touch off a verbal name or other epithet, it is only necessary that the stimulus-pattern should be sufficient to suggest

"that form." Practice and repetition make *O* more and more expert in distinguishing the several members of a series (seen at times with full foveal adequacy). The experiment comes to be, therefore, more a process of coaching in the interpretation of indistinct signs than a discovery of retinal adequacy at various removes from the fovea (7, p. 639).

This criticism of a widely used experimental procedure is perhaps more pertinent in those experiments where the stimulus figure is eccentric to the foveal fixation point, than in those where the figure is viewed foveally under the condition of increasing contrast with respect to the surround. In the latter type of experiment *O* uses that part of the retina (fovea) which is not only efficient in the resolution of fine detail, but is well adapted for and regularly employed in form perception. Even though a stimulus object might be deficient in those qualities essential to the perception of a particular form, *O* can describe confidently what is perceived at a certain stage in the perceptual process. With peripheral presentation of the stimulus object, however, a region of the retina which is capable of perceiving, at the best, gross form characteristics only, is stimulated. Further, this part of the retina is seldom used in the perception and recognition of form qualities. It is to be expected, then, that in the typical campimetric experiment *O* would tend to rely even more upon what has been learned regarding characteristics of the stimulus figure. In short, because of the inefficiency in form perception of the peripheral perceiving mechanism, *Os* would be very ready to use as much as possible any knowledge acquired with respect to the nature of the stimulus figures. Where introspective data are reported fully (15), the evidence suggests that even without any knowledge of the stimulus figures *Os* tend to fit the figure to some preconceived form. It is unfortunate that no control has been exercised over this factor of preknowledge

of, or increasing familiarity with, the stimulus figure. The failure to do so has led to the exclusion from this analysis of otherwise valuable data.

PHYSIOLOGICAL PROCESSES

Reciprocal overlap of ascending neuron chains, the presence of lateral connecting neurons, and the convergence of receptor cells upon a single ganglion cell are some of the structural properties of the visual projection system which are of importance to the statistical theory in relation to peripheral vision.

Reciprocal (20) or partially shifted (18) overlap refers to a typical arrangement of cross connections between ascending fibers resulting in the divergence of impulses at one synaptic level and re-concentration of these at another synaptic relay higher in the system. The outcome of this convergence of impulses is spatial summation. Figure 1 shows how cross connections between a "core" fiber and its collaterals give rise to a spreading of excitation at one synapse (S_1) but concentrate it at the next (S_2). Activity in the collateral fibers at the level of S_2 would tend to facilitate the core fiber, thus producing a differential rate of firing with consequent peaking of the cortical distribution of excitation. Spreading and concentration of excitation in this manner could be expected to occur both at the retinal level and higher in the system at the levels of the geniculate and cortex. Reciprocally overlapping fiber systems are more characteristic of the peripheral retina than of the foveal region, where a point projection of neurons is more closely approached. The manner in which cortical peaking occurs with foveal stimulation is more dependent upon neural recovery cycle behavior than on the neural structure described above (20).

It has been pointed out (20) that peaking of the cortical excitation pattern will occur only so long as all the later-

ally interconnected fibers potentially available for use are not operating. When all the available fibers in a group are activated fully, with no further recruitment possible, saturation occurs, i.e., summation reaches a ceiling and the peaked distribution is replaced by a plateau-type distribution. This process has been called occlusion (5) and, as Granit and Harper (12) have pointed out, the over-all effect will be determined by the balance of summation and occlusion. The addition of fresh fibers impinging upon an already maximally discharging neuron is ineffective, since the neuron is saturated. The nature of the distribution under conditions of partial and complete saturation is shown schematically in Fig. 1.

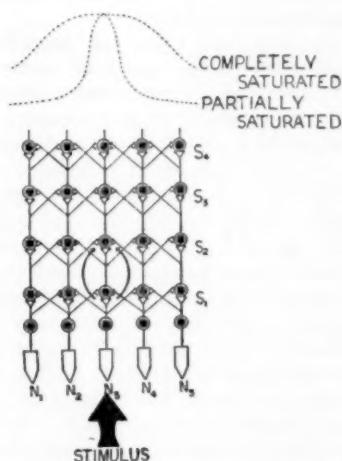


FIG. 1. Schematic representation of reciprocally overlapping fiber system. N_1 , N_2 , N_3 , N_4 , and N_5 are parallel ascending fibers, and S_1 , S_2 , S_3 , and S_4 are four levels of synaptic junction. When a stimulus activates the "core" fiber N_3 the resulting impulses tend to diverge at level S_1 and to concentrate again at level S_2 . The course of divergence and concentration of impulses is shown by the two arrows. At the top of the diagram are shown the probable shapes of the cortical excitation distributions under conditions of partial and complete saturation.

It has long been argued that the principal determinant of poor resolving power in the peripheral retina is the relatively greater ratio of receptor elements to optic nerve ganglia in this region (3, 28). When it is recalled that in parts of the peripheral retina the angular area from which receptor elements converge on to higher order neurons may be as great as $2\frac{1}{2}$ degrees, it is plain that cellular convergence would interfere considerably with visual acuity.

Marshall and Talbot (20) have suggested that convergence is not incompatible with the peaking function of reciprocal overlap, since such acuity as is lost by convergence can be regained by overlapping systems of fibers at the level of the geniculate and cortical synapses. There is no reason to question this contention as a broad generalization, and, indeed, this is the point of view which will be adopted here. It is reasonable to suppose, however, that, because of a gradient of increasing convergence from fovea to periphery, the degree to which reciprocal overlap overcomes the effects of convergence will become less as the outer periphery is approached.

Chiewitz¹ (3) has shown that the ratio of receptors to inner nuclei increases regularly from fovea to periphery. This ratio is 1.09, 10.75, 42.00, and 80.00 to 1 for distances of 1.0, 3.2, 4.6, and 6.0 mm., respectively, from the fovea. Thus, whereas in regions near the fovea the low degree of cellular convergence would not greatly interfere with peaking arising from reciprocal overlap, the much higher ratio of receptor to inner cells in the outer periphery would severely limit this function. The actual manner in which the two neural processes consequent upon reciprocal

overlap and cellular convergence interact is by no means clear. It does seem, however, that whereas reciprocal overlap spreads and later concentrates streams of impulses from a small area, resulting finally in a peaked distribution, cellular convergence tends to bring together impulses from a wide area (1), resulting in summation at a single ganglion cell. Thus the effect of convergence will be to flatten and extend the cortical pattern of excitation, whereas reciprocal overlap will serve the function of sharpening the excitation peaks. The processes of convergence of impulses from a wide area upon a single cell, and of spreading and concentration of impulses due to reciprocal overlap, are not to be thought of as entirely separate neural functions operating independently of each other. These activities are doubtless intermingled, and retinal elements such as the internuncial probably serve both functions. What is of interest to the present analysis is the degree to which each process occurs relative to the other, for upon this will depend the shape and gradient of the cortical distribution of excitation.

The reciprocally overlapping system of fibers serving the function of cortical peaking, and the increasing convergence of receptor of ganglion cells from the fovea outwards limiting this process, would appear to be the basis of dyscritic (26) or protopathic (16) and epicritic (26) vision. Certainly such a proposal fits more closely the facts than does an explanation in terms of the type and distribution of receptor cells. Parsons (26) has drawn attention to certain epicritic features of dyscritic vision. That is, the difference between these two kinds of vision is a relative one only. The increasing limitation placed upon the peaking process from fovea to periphery by the gradually increasing receptor to ganglion convergence ratio would seem to be an acceptable physio-

¹ Chiewitz's investigations were carried out without the benefit of the chrom-silver staining methods. More recent investigations indicate, however, that the findings with respect to cellular convergence ratios are accurate.

logical explanation of the two kinds of visual function.

THEORETICAL INTERPRETATION OF EXPERIMENTAL DATA

Qualitative data. The data to be treated here are those reported by Zigler *et al.* (35) and Grindley (15). Inclusion of data from Zigler *et al.* requires some justification, since this work was criticized above on the grounds that the experimental conditions permitted *O*s to grow familiar with the series of stimulus objects during the course of the experiment. Use is made of this report here since the peripheral perceiving process has been divided into a number of stages, which facilitates this analysis. Also, results from Grindley's (15) investigation, to which the above criticism does not apply, are in general agreement with those of Zigler *et al.* A further study by Galli (11) reports data which correspond to those of Zigler *et al.*

Zigler *et al.* (35), using a wide variety of stimulus patterns and employing the moving method, have shown that the perceptual process can conveniently be divided into four stages as the image moves from the outer periphery toward the fovea. The first stage occurs when *O* first becomes aware of the total stimulus object, i.e., the stimulus figure and its background. This is referred to as the "figureless field" stage. Although *O* is aware of the presence of a stimulus object in the peripheral visual field, there is no differentiation between figure and ground. The second stage, that of the "formless figure," is reached when *O* reports that he is aware of "something" upon the background, i.e., when differentiation into figure and ground first occurs. Initially, *O* usually reports the presence of a faint spot or patch which in open figures is reported as being "filled in" or solid. The third stage is that of the "form-like figure" beginning at the point at which any suggestion in

regard to shape is reported. Initially the detail of the figure becomes organized into a figure which is essentially unclassified. Only the most general characteristics of shape can be described. These include such features as "straight-sided," "rounded," and "elongated" processes. The final stage is reached when the figure is accurately perceived and correctly described or named.

As the image of the stimulus object traverses the retina from periphery to fovea, it moves from a region where there is a higher ratio of receptor to inner cells to one where this ratio is very much lower. Thus the peaking of cortical excitation consequent upon reciprocal overlap will tend to grow more marked as the fovea is more closely approached. With progressive change in the cortical pattern of excitation, from an extended and flattened distribution with a slight gradient to one which is sharpened and the gradient steep, so, subjectively, edges will become better defined and detail clearer. In the parafoveal and foveal regions, peaking becomes more pronounced as a result of the neural recovery cycle behavior of the fiber groups involved. (For a full explanation of this process see [20].) In terms of the nature of the cortical excitation pattern, the four stages of perception defined by Zigler *et al.* (35) are neurophysiologically represented by a progressive sharpening of cortical excitation peaks and increases in the steepness in the slopes of the excitation gradients. This process is represented in Fig. 2, in which is shown the probable nature of the cortical pattern of firing at the four perceptual stages described above.

Grindley (15), using the stationary method, reports a number of "errors" of perception. Among these are the perception of an inverted T figure as a triangle with apex uppermost, two squares with corners superimposed seen

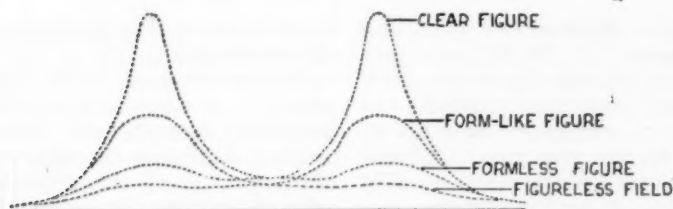


FIG. 2. Representation of cortical excitation distributions about the opposite edges of a figure for four stages of peripheral form perception. The slight increase in the height of the distributions due to facilitation within the area of overlap is shown for the "figureless field" and "formless figure" stages.

as a cross within a circle, and an open circle seen as a solid figure. Each of these phenomena can be attributed to a filling-in effect (6), correlated neurophysiologically with an extended and flattened cortical-excitation pattern. In the absence of cortical peaking an edge will appear as blurred and extended, as well as tending to merge with the background. In the case of the open circle seen as a solid circle or a disc, the extended distribution of excitation about the inner edge of one part of the circle will be continuous with the extended distribution about that part of the edge opposite to it. Figure 2 shows how, because of the flattened distribution of slight excitation gradient, this would occur. Another phenomenon of peripheral perception reported by Grindley (15) is the appearance of an open figure as an open figure but with a dark patch in the central part of the figure. Since the two cortical "ridges" of excitation about the opposite sides of a figure would tend in some cases to overlap, it is likely that those groups of fibers involved within the area of overlap would tend to facilitate each other. The outcome of such facilitation would be another peak or "ridge" of excitation at the center of the figure, although no edge or contour exists. Depending upon the size of the figure and the retinal locus of the image, this effect would lead to above-threshold excitation, thus giv-

ing the subjective impression of a patch at the center of the stimulus figure. Such a patch could occur alone or surrounded by the blurred contour of the figure, depending upon the heights of the three distributions. This process is shown diagrammatically in Fig. 2.

Blurring of the outline of a figure and the tendency for edges and contours to merge with background in peripheral vision would tend to obscure the shape of a figure, so that it would appear as a more or less circular patch or spot. This would be the case at the formless figure stage. Thus the two superimposed squares would be perceived as roughly circular, with the superimposed corners of the two squares forming a blurred cross within the circle. The blurring and filling-in effect would be responsible for an inverted T figure being perceived as a triangle with apex uppermost.

It must be stated, however, that there are a number of perceptual phenomena in peripheral vision which cannot be interpreted entirely in terms of the processes which have been outlined. If further determinants in peripheral visual perception—such as various types of eye movement (physiological nystagmus, drift, saccades), loss of fixation, disappearance of peripheral image, and O's preconceived notions of figure qualities—are also considered, then most of the phenomena are explicable.

Effect of stimulus-object size. Most investigators (15, 17, 30) are agreed that large stimulus figures are clearly seen and accurately described at a greater degree of eccentricity than are smaller figures. At a given retinal locus a small open circle would appear filled in, since the excitation distributions representing opposite edges would overlap, thus giving the subjective appearance of solidity. With larger circles, however, although the outline would still appear blurred and hazy, the circle would appear open, since the two cortical distributions would not overlap, or would overlap to the same extent as in the case of the smaller circle. In the same manner, gross aspects of form would not be obscured to the same extent as finer detail. In this respect it can be hypothesized that the degree of eccentricity from the fixation point at which a figure can be perceived clearly will depend upon the perimeter-to-area (P/A) ratio (2). This ratio defines the amount of edge detail relative to the area of the figure.

Effects of exposure time. In an examination of the effects of exposure time on peripheral visual perception, Grindley (15) varied exposure time from 0.01 to 1.00 sec., with the stimulus located at angular distances of 20, 40, and 60 degrees from the fixation point. With very short exposures the pattern was either not seen at all, or, when it was seen, it appeared as a blurred patch. With longer exposures the pattern was reported as blurred throughout the time for which it was exposed, clear at first, then growing blurred; or blurred initially and clear later. A report by Meisenheimer (21) is in agreement with these results. Unfortunately Grindley's study does not include detailed results for the three eccentric angles used. Nevertheless, the general findings for variation in exposure time are what

would be expected on the basis of the statistical theory.

The nonappearance of the stimulus object, or its appearance as a blurred patch, can again be attributed to a flattened distribution of cortical activity about the cortical representation of edges and contours. In the case of nonappearance of the stimulus pattern, the height of this distribution would be below the threshold for visual experience, and with a blurred patch slightly above that threshold. For each of these brief exposure phenomena the absence of peaking can be attributed not only to the inhibition of the mechanism of reciprocal overlap by cellular convergence, but also to an insufficiency of time for the peaked distribution to develop. Evidence for this is provided by reaction-time studies, which show that in the periphery RT increases from 0.004 sec. at 3 degrees to 0.024 sec. at 45 degrees (19, 27) indicating an increase in neural transmission time and synaptic delay. Even in the dark adapted state, when the peripheral receptors are in a very much more sensitive state, increase in reaction time compared with the fovea is insignificant (27). In the inner periphery the insufficiency of time for a peaked distribution to build up would be the principal determinant of the nonappearance of the stimulus object. In the outer periphery this time factor would operate along with the inhibitory effects of convergence.

The brief appearance of the figure at the beginning of the exposure, followed by blurring for the remainder of the exposure, is probably attributable to a saturation process already described. This process is described more fully in the next section dealing with "fixation blindness." Depending upon the retinal locus of the image and the exposure time involved, initial blurring followed by a clear image is due, probably, to the time necessary for the building up of

cortical peaks of activity. It would be expected that this phenomenon would be more characteristic of the outer peripheral region.

Whether the stimulus pattern is blurred throughout the time of exposure, appears clear at first, and then fades to become blurred or to disappear completely, or vice versa, will be a complex function of other factors as well as those relating to retinal and cortical processes. These include retinal locus of the image, stimulus size, shape, and intensity, together with constancy of fixation, knowledge concerning the stimulus pattern, and eye movements. So far, the conditions under which an object is clear or blurred, or passes from one of these states to the other, have not been specified. There is obviously a considerable need for experimentation to determine under what conditions these various phenomena occur.

Disappearance of the peripheral image. Fading of the peripheral image, first reported by Troxler (33), and sometimes referred to as "fixation blindness" (14), occurs usually after two to three seconds of fixation, and is limited to the peripheral retina. This effect occurs more rapidly at large peripheral angles and with greater stimulus intensities, but less rapidly with large stimulus objects. Restoration of the image can be brought about by brief loss of fixation, or by rapid removal and replacement of the stimulus figure. Dunlap (9) reported that reappearance of the image occurs spontaneously, sometimes in the form of the stimulus object itself, sometimes as a darker spot, and occasionally as a coronal effect surrounding the position where the stimulus object was last seen. Dunlap reports also that: "A haze or blur of diffused light was discernible, covering a considerable part of the visual field around the spot, and this haze of light

remained after the brighter spot had disappeared" (9, p. 204).

Discrimination between a figure and its background is dependent upon the perception of an edge or contour separating the two. The cortical correlate of the clear perception of an edge is a sharpened peak to the cortical excitation pattern and a steep excitation gradient. In the peripheral retina, as has been pointed out, this sharpening is dependent upon a reciprocally overlapping system of fibers. When such a system becomes saturated the cortical excitation distribution becomes flattened, and the excitation gradient less steep (Fig. 1). The subjective outcome of saturation would be the blurring and obscuring of edges and borders, with consequent loss of clear differentiation between figure and ground. Indeed Dunlap's (9) description of "a haze or blur of diffused light" is precisely how an edge would be expected to appear under conditions of saturation.

A process of saturation in the manner suggested is well supported by experimental evidence. The rapid onset of the process and a negatively accelerated course (14) is to be expected from the structural arrangement of fibers responsible for peaking. A more intense stimulus would result in more rapid saturation of the group of fibers involved, and a large stimulus object would be less likely to be obscured by edge blurring and haziness than would a smaller one. Brief loss of fixation or removal of the stimulus would permit the saturated fiber group to recover, and so restore the image.

Spontaneous reappearance of the image is more difficult to account for. Figure 1 shows, however, that, although the cortical excitation pattern under conditions of saturation is flattened and the gradient slight, the height of the distribution remains above threshold for perception. That is to say, although con-

tour is obscured by blurring about the edges, resulting in virtual loss of the figure, the height of the distribution remains at threshold level. Assuming nystagmatic eye movement and drift, however, further fibers will be activated, causing the point of maximal cortical excitation to shift, and thus resulting in the brief reappearances of the image before these, too, become saturated. Since the distribution has remained above threshold, activation of further fibers, resulting in the definition of edges, would readily result in the reappearance of the image.

An experiment. The theory of peripheral form perception which has been outlined possesses the advantage of permitting relatively precise predictions to be made regarding the changes in the appearance of a stimulus pattern as the distance of the image from the fovea is decreased. An as yet unpublished investigation carried out by the writer was designed to test some of the predictions and is worth quoting in this context.

The six figures used are shown in Fig. 3. These were presented by means of an exposure device mounted on a Stoelting campimeter. The *O*s were quite unfamiliar with the figures, and no comment was made by *E* after a description of a particular test object had been offered. The *O*s both described and drew what they saw at each angle of presentation.

Generally it was predicted that in the outer periphery the figures would be seen as dark, almost formless, patches on a whitish ground. Closer to the fovea, small edge indentations would be filled in and corners rounded by blurring at the edges. White spaces enclosed within the figure would tend to be seen

as continuous with the rest of the figure, due to the filling-in effect. Forms 2, 5, and 6 would be seen at some stage as rectangles before the emergence of detail. Forms 1 and 3 would be seen as roughly triangular, and Form 4 seen first as a hazy rounded patch, then as diamond-shaped and finally as a blurred cross. In all cases these predictions have been correct. For example, the inverted *L* shape was seen first by one *O* at 70 degrees as a dark blur with a flattened top, at 50 degrees as a dark triangle with apex downward, and at 30 degrees as a boomerang shape. Again, Form 6 was perceived first as a blurred dark patch, and at between 50 and 30 degrees as a hollow square before it emerged clearly.

SUMMARY AND CONCLUSIONS

A theory of form perception in the peripheral retina, based upon the statistical theory of neurophysiological activity in the visual primary projection system, has been presented. The basic assumption in this theory is that an edge, border or contour has as its cortical correlate a near-Gaussian distribution of excitation in area 17. The sharpness of the peak of excitation of this distribution, and the slope of the gradient, determine the clarity with which form is perceived.

Certain methodological problems relating to the familiarity of *O* with the stimulus objects have been examined. Attention is drawn to the tendency of *O* merely to recognize and designate, as opposed to "seeing" and describing, the stimulus object when the experimental conditions are such as to allow *O* to become familiar with the stimulus objects.

It is suggested that in the peripheral retina the reciprocally overlapping systems of fibers give rise to a peaking of the cortical excitation pattern, whereas convergence of the receptor layer upon inner retinal cells tends to limit or in-

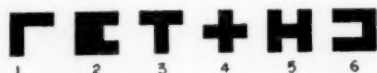


FIG. 3. Six forms used in experiment.

hibit this process. Since convergence increases from the fovea to the outer periphery, the latter is much less efficient in form perception. It is proposed that the processes consequent upon reciprocal overlap and cellular convergence form the basis of what has been called dyscritic and epicritic vision.

Data drawn from various sources have been examined and interpreted as evidence for the theoretical proposals. These data include qualitative aspects of peripheral form perception, effects of stimulus-object size and exposure time, and peripheral "fixation blindness."

An experiment designed to test some predictions of this theory is described briefly.

The statistical theory has now been evoked to deal with such diverse visual phenomena as visual acuity (20, 32), figural aftereffects (24), movement perception (23), and foveal form perception (6). With each of these phenomena the theory has dealt satisfactorily with the greater part of the experimental data. It is possibly pertinent to add that a not dissimilar theory has been proposed in regard to somesthetic sensitivity (29).

Because knowledge of the neurophysiological processes of the visual system is still incomplete, it is inevitable that many of the theoretical proposals are speculative. The theory does have, however, the important virtue of permitting readily testable hypotheses to be set up and subjected to physiological and psychological experiment. Until this is done, the validity of the statistical theory, and such modifications that may prove necessary to increase its validity, must remain uncertain.

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INTERFERENCE AND FORGETTING

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I know of no one who seriously maintains that interference among tasks is of no consequence in the production of forgetting. Whether forgetting is conceptualized at a strict psychological level or at a neural level (e.g., neural memory trace), some provision is made for interference to account for at least some of the measured forgetting. The many studies on retroactive inhibition are probably responsible for this general agreement that interference among tasks must produce a sizable proportion of forgetting. By introducing an interpolated interfering task very marked decrements in recall can be produced in a few minutes in the laboratory. But there is a second generalization which has resulted from these studies, namely, that most forgetting must be a function of the learning of tasks which interfere with that which has already been learned (19). Thus, if a single task is learned in the laboratory and retention measured after a week, the loss has been attributed to the interference from activities learned outside the laboratory during the week. It is this generalization with which I am concerned in the initial portions of this paper.

Now, I cannot deny the data which show large amounts of forgetting produced by an interpolated list in a few minutes in the laboratory. Nor do I deny that this loss may be attributed to interference. But I will try to show

that use of retroactive inhibition as a paradigm of forgetting (via interference) may be seriously questioned. To be more specific: if a subject learns a single task, such as a list of words, and retention of this task is measured after a day, a week, or a month, I will try to show that very little of the forgetting can be attributed to an interfering task learned outside the laboratory during the retention interval. Before pursuing this further, I must make some general comments by way of preparation.

Whether we like it or not, the experimental study of forgetting has been largely dominated by the Ebbinghaus tradition, both in terms of methods and materials used. I do not think this is due to sheer perversity on the part of several generations of scientists interested in forgetting. It may be noted that much of our elementary knowledge can be obtained only by rote learning. To work with rote learning does not mean that we are thereby not concerning ourselves with phenomena that have no counterparts outside the laboratory. Furthermore, the investigation of these phenomena can be handled by methods which are acceptable to a science. As is well known, there are periodic verbal revolts against the Ebbinghaus tradition (e.g., 2, 15, 22). But for some reason nothing much ever happens in the laboratory as a consequence of these revolts. I mention these matters neither by way of apology nor of justification for having done some research in rote learning, but for two other reasons. First, it may very well be true, as some have suggested (e.g., 22), that studies of memory in the Ebbinghaus tradition are not getting at all of the important

¹ Address of the president, Midwestern Psychological Association, St. Louis, Missouri, May, 1956.

² Most of the data from my own research referred to in this paper were obtained from work done under Contract N7 onr-45008, Project NR 154-057, between Northwestern University and The Office of Naval Research.

phenomena of memory. I think the same statement—that research has not got at all of the important processes—could be made about all areas in psychology; so that the criticism (even if just) should not be indigenous to the study of memory. Science does not deal at will with all natural events. Science deals with natural events only when ingenuity in developing methods and techniques of measurement allow these events to be brought within the scope of science. If, therefore, the studies of memory which meet scientific acceptability do not tap all-important memorial processes, all I can say is that this is the state of the science in the area at the moment. Secondly, because the bulk of the systematic data on forgetting has been obtained on rote-learned tasks, I must of necessity use such data in discussing interference and forgetting.

Returning to the experimental situation, let me again put in concrete form the problem with which I first wish to deal. A subject learns a single task, such as a list of syllables, nouns, or adjectives. After an interval of time, say, 24 hours, his retention of this list is measured. The explanatory problem is what is responsible for the forgetting which commonly occurs over the 24 hours. As indicated earlier, the studies of retroactive inhibition led to the theoretical generalization that this forgetting was due largely to interference from other tasks learned during the 24-hour retention interval. McGeoch (20) came to this conclusion, his last such statement being made in 1942. I would, therefore, like to look at the data which were available to McGeoch and others interested in this matter. I must repeat that the kind of data with which I am concerned is the retention of a list without formal interpolated learning introduced. The interval of retention with which I am going to deal in this, and several subsequent analyses, is 24 hours.

First, of course, Ebbinghaus' data were available and in a sense served as the reference point for many subsequent investigations. In terms of percentage saved in relearning, Ebbinghaus showed about 65 per cent loss over 24 hours (7). In terms of recall after 24 hours, the following studies are representative of the amount forgotten: Youtz, 88 per cent loss (37); Luh, 82 per cent (18); Krueger, 74 per cent (16); Hovland, 78 per cent (11); Cheng, 65 per cent and 84 per cent (6); Lester, 65 per cent (17). Let us assume as a rough average of these studies that 75 per cent forgetting was measured over 24 hours. In all of these studies the list was learned to one perfect trial. The percentage values were derived by dividing the total number of items in the list into the number lost and changing to a percentage. Thus, on the average in these studies, if the subject learned a 12-item list and recalled three of these items after 24 hours, nine items (75 per cent) were forgotten.

The theory of interference as advanced by McGeoch, and so far as I know never seriously challenged, was that during the 24-hour interval subjects learned something outside the laboratory which interfered with the list learned in the laboratory. Most of the materials involved in the investigations cited above were nonsense syllables, and the subjects were college students. While realizing that I am viewing these results in the light of data which McGeoch and others did not have available, it seems to me to be an incredible stretch of an interference hypothesis to hold that this 75 per cent forgetting was caused by something which the subjects learned outside the laboratory during the 24-hour interval. Even if we agree with some educators that much of what we teach our students in college is nonsense, it does not seem to be the kind

of learning that would interfere with nonsense syllables.

If, however, this forgetting was not due to interference from tasks learned outside the laboratory during the retention interval, to what was it due? I shall try to show that most of this forgetting was indeed produced by interference—not from tasks learned outside the laboratory, but from tasks learned previously in the laboratory. Following this I will show that when interference from laboratory tasks is removed, the amount of forgetting which occurs is relatively quite small. It then becomes more plausible that this amount could be produced by interference from tasks learned outside the laboratory, although, as I shall also point out, the interference very likely comes from prior, not interpolated, learning.

In 1950 a study was published by Mrs. Greenberg and myself (10) on retention as a function of stage of practice. The orientation for this study was crassly empirical; we simply wanted to know if subjects learn how to recall in the same sense that they learn how to learn. In the conditions with which I am concerned, naive subjects learned a list of ten paired adjectives to a criterion of eight out of ten correct on a single trial. Forty-eight hours later this list was recalled. On the following day, these same subjects learned a new list to the same criterion and recalled it after 48 hours. This continued for two additional lists, so that the subjects had learned and recalled four lists, but the learning and recall of each list was complete before another list was learned. There was low similarity among these lists as far as conventional symptoms of similarity are concerned. No words were repeated and no obvious similarities existed, except for the fact that they were all adjectives and a certain amount of similarity among prefixes, suffixes, and so on must inevitably occur. The

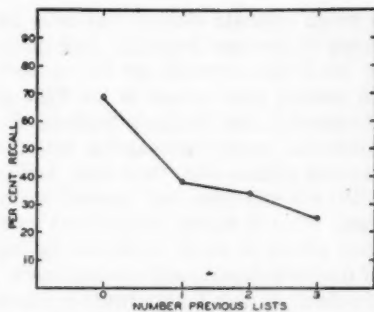


FIG. 1. Recall of paired adjectives as a function of number of previous lists learned (10).

recall of these four successive lists is shown in Fig. 1.

As can be seen, the more lists that are learned, the poorer the recall, from 69 per cent recall of the first list to 25 per cent recall of the fourth list. In examining errors at recall, we found a sufficient number of intrusion responses from previous lists to lead us to suggest that the increasing decrements in recall were a function of proactive interference from previous lists. And, while we pointed out that these results had implications for the design of experiments on retention, the relevance to an interference theory of forgetting was not mentioned.

Dr. E. J. Archer has made available to me certain data from an experiment which still is in progress and which deals with this issue. Subjects learned lists of 12 serial adjectives to one perfect trial and recalled them after 24 hours. The recall of a list always took place prior to learning the next list. The results for nine successive lists are shown in Fig. 2. Let me say again that there is no laboratory activity during the 24-hour interval; the subject learns a list, is dismissed from the laboratory, and returns after 24 hours to recall the list. The percentage of recall falls from 71 per cent for the first list to 27 per cent for the ninth.

In summarizing the more classical data on retention above, I indicated that

a rough estimate showed that after 24 hours 75 per cent forgetting took place, or recall was about 25 per cent correct. In viewing these values in the light of Greenberg's and Archer's findings, the conclusion seemed inescapable that the classical studies must have been dealing with subjects who had learned many lists. That is to say, the subjects must have served in many conditions by use of counterbalancing and repeated cycles. To check on this I have made a search of the literature on the studies of retention to see if systematic data could be compiled on this matter. Preliminary work led me to establish certain criteria for inclusion in the summary to be presented. First, because degree of learning is such an important variable, I have included only those studies in which degree of learning was one perfect recitation of the list. Second, I have included only studies in which retention was measured after 24 hours. Third, I have included only studies in which recall measures were given. (Relearning measures add complexities with which I do not wish to deal in this paper.) Fourth, the summary includes only material learned by relatively massed practice. Finally, if an investigator had two or more conditions which met these criteria, I averaged the values presentation

in this paper. Except for these restrictions, I have used all studies I found (with an exception to be noted later), although I do not pretend to have made an exhaustive search. From each of these studies I got two facts: first, the percentage recalled after 24 hours, and second, the average number of previous lists the subjects had learned before learning the list on which recall after 24 hours was taken. Thus, if a subject had served in five experimental conditions via counterbalancing, and had been given two practice lists, the average number of lists learned before learning the list for which I tabulated the recall was four. This does not take into account any previous experiments in rote learning in which the subject might have served.

For each of these studies the two facts, average number of previous lists learned and percentage of recall, are related as in Fig. 3. For example, consider the study by Youtz. This study was concerned with Jost's law, and had several degrees of learning, several lengths of retention interval, and the subjects served in two cycles. Actually, there were 15 experimental conditions and each subject was given each condition twice. Also, each subject learned six practice lists before starting the experimental conditions. Among the 15 conditions was one in which the learning of the syllables was carried to one perfect recitation and recall was taken after 24 hours. It is this particular condition in which I am interested. On the average, this condition would have been given at the time when the subject had learned six practice lists and 15 experimental lists, for a total of 21 previous lists.

The studies included in Fig. 3 have several different kinds of materials, from geometric forms to nonsense syllables to nouns; they include both paired-associate and serial presentation, with differ-

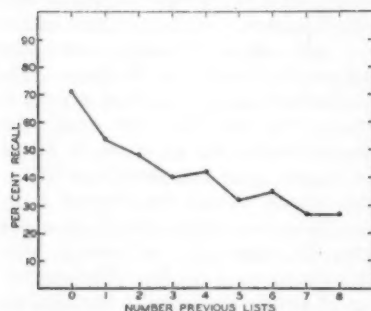


FIG. 2. Recall of serial adjective lists as a function of number of previous lists learned. Unpublished data, courtesy of Dr. E. J. Archer.

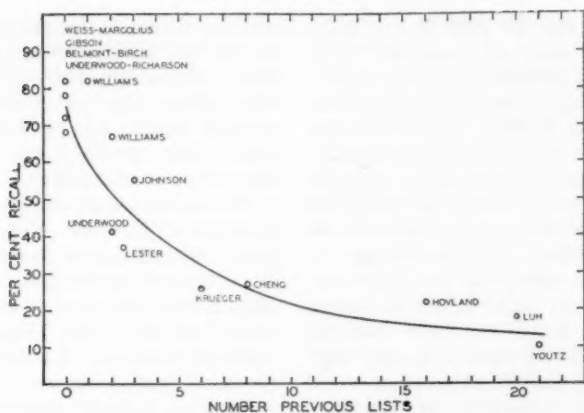


FIG. 3. Recall as a function of number of previous lists learned as determined from a number of studies. From left to right: Weiss and Margolius (35), Gibson (9), Belmont and Birch (3), Underwood and Richardson (33), Williams (36), Underwood (27, 28, 29, 30), Lester (17), Johnson (14), Krueger (16), Cheng (6), Hovland (11), Luh (18), Youtz (37).

ent speeds of presentation and different lengths of lists. But I think the general relationship is clear. The greater the number of previous lists learned the greater the forgetting. I interpret this to mean that the greater the number of previous lists the greater the *proactive* interference. We know this to be true (26) for a formal proactive-inhibition paradigm; it seems a reasonable interpretation for the data of Fig. 3. That there are minor sources of variance still involved I do not deny. Some of the variation can be rationalized, but that is not the purpose of this report. The point I wish to make is the obvious one of the relationship between number of previous lists learned—lists which presumably had no intentionally built-in similarity—and amount of forgetting. If you like to think in correlational terms, the rank-order correlation between the two variables is $-.91$ for the 14 points of Fig. 3.

It may be of interest to the historian that, of the studies published before 1942 which met the criteria I imposed, I did not find a single one in which sub-

jects had not been given at least one practice task before starting experimental conditions, and in most cases the subjects had several practice lists and several experimental conditions. Gibson's study (1942) was the first I found in which subjects served in only one condition and were not given practice tasks. I think it is apparent that the design proclivities of the 1920s and 1930s have been largely responsible for the exaggerated picture we have had of the rate of forgetting of rote-learned materials. On the basis of studies performed during the 1920s and 1930s, I have given a rough estimate of forgetting as being 75 per cent over 24 hours, recall being 25 per cent. On the basis of modern studies in which the subject has learned no previous lists—where there is no proactive inhibition from previous laboratory tasks—a rough estimate would be that forgetting is 25 per cent; recall is 75 per cent. The values are reversed. (If in the above and subsequent discussion my use of percentage values as if I were dealing with a cardinal or extensive scale is dis-

turbing, I will say only that it makes the picture easier to grasp, and in my opinion no critical distortion results.)

Before taking the next major step, I would like to point out a few other observations which serve to support my general point that proactive inhibition from laboratory tasks has been the major cause of forgetting in the more classical studies. The first illustration I shall give exemplifies the point that when subjects have served in several conditions, forgetting after relatively short periods of time is greater than after 24 hours if the subject has served in only one condition. In the Youtz study to which I have already referred, other conditions were employed in which recall was taken after short intervals. After 20 minutes recall was 74 per cent, about what it is after 24 hours if the subject has not served in a series of conditions. After two hours recall was 32 per cent. In Ward's (34) well-known reminiscence experiment, subjects who on the average had learned ten previous lists showed a recall of only 64 per cent after 20 minutes.

In the famous Jenkins-Dallenbach (13) study on retention following sleep and following waking, two subjects were used. One subject learned a total of 61 lists and the other 62 in addition to several practice lists. Roughly, then, if the order of the conditions was randomized, approximately 30 lists had been learned prior to the learning of a list for a given experimental condition. Recall after eight waking hours for one subject was 4 per cent and for the other 14 per cent. Even after sleeping for eight hours the recall was only 55 per cent and 58 per cent.

I have said that an interpolated list can produce severe forgetting. However, in one study (1), using the A-B, A-C paradigm for original and interpolated learning, but using subjects who had never served in any previous con-

ditions, recall of the original list was 46 per cent after 48 hours, and in another comparable study (24), 42 per cent. Thus, the loss is not nearly as great as in the classical studies I have cited where there was no interpolated learning in the laboratory.

My conclusion at this point is that, in terms of the gross analysis I have made, the amount of forgetting which might be attributed to interference from tasks learned outside the laboratory has been "reduced" from 75 per cent to about 25 per cent. I shall proceed in the next section to see if we have grounds for reducing this estimate still more. In passing on to this section, however, let me say that the study of factors which influence proactive inhibition in these counterbalanced studies is a perfectly legitimate and important area of study. I mention this because in the subsequent discussion I am going to deal only with the case where a subject has learned a single list in the laboratory, and I do not want to leave the impression that we should now and forevermore drop the study of interference produced by previous laboratory tasks. Indeed, as will be seen shortly, it is my opinion that we should increase these studies for the simple reason that the proactive paradigm provides a more realistic one than does the retroactive paradigm.

When the subject learns and recalls a single list in the laboratory, I have given an estimate of 25 per cent as being the amount forgotten over 24 hours. When, as shown above, we calculate percentage forgotten of lists learned to one perfect trial, the assumption is that had the subjects been given an immediate recall trial, the list would have been perfectly recalled. This, of course, is simply not true. The major factor determining how much error is introduced by this criterion-percentage method is probably the difficulty of the task. In general,

the overestimation of forgetting by the percentage method will be directly related to the difficulty of the task. Thus, the more slowly the learning approaches a given criterion, the greater the drop on the trial immediately after the criterion trial. Data from a study by Runquist (24), using eight paired adjectives (a comparatively easy task), shows that amount of forgetting is overestimated by about 10 per cent. In a study (32) using very difficult consonant syllables, the overestimation was approximately 20 per cent. To be conservative, assume that on the average the percentage method of reporting recall overestimates the amount forgotten by 10 per cent. If we subtract this from the 25 per cent assumed above, the forgetting is now re-estimated as being 15 per cent over 24 hours. That is to say, an interference theory, or any other form of theory, has to account for a very small amount of forgetting as compared with the amount traditionally cited.

What are the implications of so greatly "reducing" the amount of forgetting? There are at least three implications which I feel are worth pointing out. First, if one wishes to hold to an interference theory of forgetting (as I do), it seems plausible to assert that this amount of forgetting could be produced from learning which has taken place outside of the laboratory. Furthermore, it seems likely that such interference must result primarily from proactive interference. This seems likely on a simple probability basis. A 20-year-old college student will more likely have learned something during his 20 years prior to coming to the laboratory that will interfere with his retention than he will during the 24 hours between the learning and retention test. However, the longer the retention interval the more important will retroactive in-

terference become relative to proactive interferences.

The second implication is that these data may suggest greater homogeneity or continuity in memorial processes than hitherto supposed. Although no one has adequately solved the measurement problem of how to make comparisons of retention among conditioned responses, prose material, motor tasks, concept learning, and rote-learned tasks, the gross comparisons have indicated that rote-learned tasks were forgotten much more rapidly than these other tasks. But the rote-learning data used for comparison have been those derived with the classical design in which the forgetting over 24 hours is approximately 75 per cent. If we take the revised estimate of 15 per cent, the discrepancies among tasks become considerably less.

The third implication of the revised estimate of rate of forgetting is that the number of variables which appreciably influence rate of forgetting must be sharply limited. While this statement does not inevitably follow from the analyses I have made, the current evidence strongly supports the statement. I want to turn to the final section of this paper which will consist of a review of the influence of some of the variables which are or have been thought to be related to rate of forgetting. In considering these variables, it is well to keep in mind that a variable which produces only a small difference in forgetting is important if one is interested in accounting for the 15 per cent assumed now as the loss over 24 hours. If appropriate for a given variable, I will indicate where it fits into an interference theory, although in no case will I endeavor to handle the details of such a theory.

Time. Passage of time between learning and recall is the critical defining variable for forgetting. Manipulation of this variable provides the basic data for

which a theory must account. Previously, our conception of rate of forgetting as a function of time has been tied to the Ebbinghaus curve. If the analysis made earlier is correct, this curve does not give us the basic data we need. In short, we must start all over and derive a retention curve over time when the subjects have learned no previous materials in the laboratory. It is apparent that I expect the fall in this curve over time to be relatively small.

In conjunction with time as an independent variable, we must, in explanations of forgetting, consider why sleep retards the processes responsible for forgetting. My conception, which does not really explain anything, is that since forgetting is largely produced by proactive interference, the amount of time which a subject spends in sleep is simply to be subtracted from the total retention interval when predicting the amount to be forgotten. It is known that proactive interference increases with passage of time (5); sleep, I believe, brings to a standstill whatever these processes are which produce this increase.

Degree of learning. We usually say that the better or stronger the learning the more or better the retention. Yet, we do not know whether or not the *rate* of forgetting differs for items of different strength. The experimental problem is a difficult one. What we need is to have a subject learn a single association and measure its decline in strength over time. But this is difficult to carry out with verbal material, since almost of necessity we must have the subject learn a series of associations, to make it a reasonable task. And, when a series of associations are learned, complications arise from interaction effects among associations of different strength. Nevertheless, we may expect, on the basis of evidence from a wide variety of studies, that given a constant degree of similarity, the effective interference varies as

some function of the strength of associations.

Distribution of practice. It is a fact that distribution of practice during acquisition influences retention of verbal materials. The facts of the case seem to be as follows. If the subject has not learned previous lists in the laboratory, massed practice gives equal or better retention than does distributed practice. If, on the other hand, the subject has learned a number of previous lists, distributed practice will facilitate retention (32). We do not have the theoretical solution to these facts. The point I wish to make here is that whether or not distribution of learning inhibits or facilitates retention depends upon the amount of interference from previous learning. It is reasonable to expect, therefore, that the solution to the problem will come via principles handling interference in general. I might also say that a theoretical solution to this problem will also provide a solution for Jost's laws.

Similarity. Amount of interference from other tasks is closely tied to similarity. This similarity must be conceived of as similarity among materials as such and also situational similarity (4). When we turn to similarity within a task, the situation is not quite so clear. Empirically and theoretically (8) one would expect that intratask similarity would be a very relevant variable in forgetting. As discussed elsewhere (31), however, variation in intratask similarity almost inevitably leads to variations in intertask similarity. We do know from a recent study (33) that with material of low meaningfulness forgetting is significantly greater with high intralist similarity than with low. While the difference in magnitude is only about 8 per cent, when we are trying to account for a total loss of 15 per cent, this amount becomes a major matter.

Meaningfulness. The belief has long

been held that the more meaningful the material the better the retention—the less the forgetting. Osgood (21) has pointed out that if this is true it is difficult for an interference theory to handle. So far as I know, the only direct test of the influence of this variable is a recent study in which retention of syllables of 100 per cent association value was compared with that of zero association value (33). There was no difference in the recall of these syllables. Other less precise evidence would support this finding when comparisons are made among syllables, adjectives, and nouns, as plotted in Fig. 3. However, there is some evidence that materials of very low meaningfulness are forgotten more rapidly than nonsense syllables of zero association value. Consonant syllables, both serial (32) and paired associates (unpublished), show about 50 per cent loss over 24 hours. The study using serial lists was the one mentioned earlier as knowingly omitted from Fig. 3. These syllables, being extremely difficult to learn, allow a correction of about 20 per cent due to criterion overestimation, but even with this much correction the forgetting (30 per cent) is still appreciably more than the estimate we have made for other materials. To invoke the interference theory to account for this discrepancy means that we must demonstrate how interference from other activities could be greater for these consonant syllables than for nonsense syllables, nouns, adjectives, and other materials. Our best guess at the present time is that the sequences of letters in consonant syllables are contrary to other well-established language habits. That is to say, letter sequences which commonly occur in our language are largely different from those in consonant syllables. As a consequence, not only are these consonant syllables very difficult to learn, but forgetting is accelerated by proactive interference from previously

well-learned letter sequences. If subsequent research cannot demonstrate such a source of interference, or if some other source is not specified, an interference theory for this case will be in some trouble.

Affectivity. Another task dimension which has received extensive attention is the affective tone of the material. I would also include here the studies attaching unpleasant experiences to some items experimentally and not to others, and measuring retention of these two sets of items. Freud is to a large extent responsible for these studies, but he cannot be held responsible for the malformed methodology which characterizes so many of them. What can one say by way of summarizing these studies? The only conclusion that I can reach is a statistical one, namely, that the occasional positive result found among the scores of studies is about as frequent as one would expect by sampling error, using the 5 per cent level of confidence. Until a reliable body of facts is established for this variable and associated variables, no theoretical evaluation is possible.

Other variables. As I indicated earlier, I will not make an exhaustive survey of the variables which may influence rate of forgetting. I have limited myself to variables which have been rather extensively investigated, which have immediate relevance to the interference theory, or for which reliable relationships are available. Nevertheless, I would like to mention briefly some of these other variables. There is the matter of *warm-up* before recall; some investigators find that this reduces forgetting (12); others, under as nearly replicated conditions as is possible to obtain, do not (23). Some resolution must be found for these flat contradictions. It seems perfectly reasonable, however, that inadequate set or context differences could reduce recall. Indeed, an

interference theory would predict this forgetting if the set or context stimuli are appreciably different from those prevailing at the time of learning. In our laboratory we try to reinstate the learning set by careful instructions, and we simply do not find decrements that might be attributed to inadequate set. For example, in a recent study (33) subjects were given a 24-hour recall of a serial list after learning to one perfect trial. I think we would expect that the first item in the list would suffer the greatest decrement due to inadequate set, yet this item showed only .7 per cent loss. But let it be clear that when we are attempting to account for the 15 per cent loss over 24 hours, we should not overlook any possible source for this loss.

Thus far I have not said anything about forgetting as a function of characteristics of the subject, that is, the personality or intellectual characteristics. As far as I have been able to determine, there is not a single valid study which shows that such variables have an appreciable influence on forgetting. Many studies have shown differences in learning as a function of these variables, but not differences in rate of forgetting. Surely there must be some such variables. We do know that if subjects are severely insulted, made to feel stupid, or generally led to believe that they have no justification for continued existence on the earth just before they are asked to recall, they will show losses (e.g., 25, 38), but even the influence of this kind of psychological beating is short lived. Somehow I have never felt that such findings need explanation by a theory used to explain the other facts of forgetting.

Concerning the causes of forgetting, let me sum up in a somewhat more dogmatic fashion than is probably justified. One of the assumptions of science is finite causality. Everything cannot in-

fluence everything else. To me, the most important implication of the work on forgetting during the last ten years is that this work has markedly *reduced* the number of variables related to forgetting. Correspondingly, I think the theoretical problem has become simpler. It is my belief that we can narrow down the cause of forgetting to interference from previously learned habits, from habits being currently learned, and from habits we have yet to learn. The amount of this interference is primarily a function of similarity and associative strength, the latter being important because it interacts with similarity.

SUMMARY

This paper deals with issues in the forgetting of rote-learned materials. An analysis of the current evidence suggests that the classical Ebbinghaus curve of forgetting is primarily a function of interference from materials learned previously in the laboratory. When this source of interference is removed, forgetting decreases from about 75 per cent over 24 hours to about 25 per cent. This latter figure can be reduced by at least 10 per cent by other methodological considerations, leaving 15 per cent as an estimate of the forgetting over 24 hours. This estimate will vary somewhat as a function of intratask similarity, distributed practice, and with very low meaningful material. But the overall evidence suggests that similarity with other material and situational similarity are by far the most critical factors in forgetting. Such evidence is consonant with a general interference theory, although the details of such a theory were not presented here.

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A CRITIQUE OF KÖHLER'S THEORY OF ASSOCIATION

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In 1941 Köhler published a paper entitled, "On the nature of associations" (3). After restating the traditional opposition between *association* and *organization*, Köhler presented what he considered to be decisive experimental evidence in favor of a Gestalt theory of learning. Köhler's findings do, indeed, appear to raise serious difficulties for association theory. We shall try to show that these difficulties are only apparent, and that the experimental facts can be fully understood in terms of principles of associative learning.¹

Köhler's theory of association. In his critical analysis of the process of association Köhler focuses on the "relation which the characteristics of one item bear to the characteristics of the other" (3, p. 490). According to Köhler, this relation is a matter of indifference for association theory. The formation of connections is a function of contiguity, regardless of the nature of the items. For Gestalt theory, on the other hand, the relation between the items is a crucial determinant of both learning and retention. The relation between the items is critical, since learning is considered to be a matter of perceptual organization. Perceptual organizations ("primary experiences"), in turn, are preserved in the nervous system as memory traces. If the perceptual experience is "unitary" by virtue of the proximity or similarity of the compo-

nent parts, the corresponding memory trace has the same "unitary" character.

Given these assumptions, the fact of association by contiguity can be reinterpreted as a special case of organization. Suppose a unitary trace has been formed, and part of such a trace is reactivated at a later time. "Because of the unitary character of the trace, this excitation will spread more easily within the trace than to other regions of the tissue" (3, p. 493). Association, Köhler concluded, is "simply coherence within the unitary trace of a unitary experience." Association by contiguity is, therefore, a secondary concept subsidiary to the more general concept of organization.

Experimental evidence for Köhler's theory. This argument has a clear-cut empirical implication. Any principle of perceptual organization is necessarily also a principle of association. In his experiments Köhler chose the variable of *similarity* to demonstrate the validity of this implication. Similarity favors perceptual grouping, i.e., similar items tend to form unitary configurations. By the same token, similarity of cue and response should favor the formation of connections in paired-associate learning. What appears to be positive evidence for the hypothesis was obtained in a series of related experiments.

In Köhler's first study, two series of six pairs each were used: (a) a series of *homogeneous* pairs consisting of two pairs of nonsense syllables, two pairs of two-place numbers, and two pairs of nonsense figures; (b) a series of *heterogeneous* pairs containing one of each of the six possible combinations of the three types of items. Two presentations

¹ Professor Köhler reaffirmed the crucial significance which he attaches to his findings during his Hitchcock Lectures, delivered at the University of California, Berkeley, in the spring of 1955. It was because these studies were again offered as decisive proof for his theory of association that our interest in this problem was aroused.

of the series (apparently in a constant serial order for a given *S*) were followed by one test trial using the method of paired associates. All *Ss* learned both series, with the order of the two series counterbalanced. A substantially higher percentage of homogeneous than heterogeneous items was recalled. The second experiment introduced two modifications. First, the response items in the two series were identical, which they had not been in the first study. Second, independent groups learned the homogeneous and heterogeneous series. Again, homogeneous items were recalled much better than heterogeneous items. The third experiment was designed to show that the critical variable was the similarity between the members of *individual pairs* rather than the composition of the total series. Homogeneous and heterogeneous pairs were combined in one series. As before, recall was considerably higher for homogeneous than heterogeneous items. In the final experiment, homogeneity and heterogeneity were defined in terms of the physical resemblance of the members of a pair. In the heterogeneous series, the cue and response members differed with respect to size and color; in the homogeneous series there were no such differences in physical appearance. Such "extrinsic" similarity of the items again favored recall, although the differences were smaller than in the earlier experiments in which the "intrinsic" similarity of the items was varied.

Did Köhler test his own hypothesis? Köhler's theoretical analysis, his derivation of the empirical predictions, and his experimental treatment are all open to serious questions. The central point of Köhler's position is the assumption of an isomorphic relation between perceptual organization and memory trace. Throughout his analysis, Köhler equates the arousal of memory traces with the spread of excitation through "ganglionic

tissue." There is no doubt, therefore, that a *spatial* correspondence between percept and memory trace is assumed. Serious difficulties arise in the application of this model of cortical projection to the dimensions of similarity manipulated in the experiments.

In the first three experiments, cue-response pairs were considered "similar" or "homogeneous" if both members of a pair belonged to the same meaning class, i.e., syllables, numbers, or geometric figures. Such classifications are arbitrary conventions. As *geometric patterns*, there are numbers which are more similar to some letters than they are to other numbers. Consider, for example, the number 0 (zero) and the letter *O*; or the number 1 (one) and the letter *I*. Clearly, Köhler chose to define perceptual similarity in terms of the *arbitrary, learned* categories to which the individual items can be assigned. Such a conception of similarity is not very different from Thorndike's principle of "belongingness," which Köhler rejects as inadequate because it refers to arbitrary rather than intrinsic relationships among the stimuli. In practice, similarity for Köhler means, "this goes with that," just as it did for Thorndike. In addition, Köhler is forced to assume that the cortical traces of items belonging to the same conventional class interact more readily in the "ganglionic tissue" than do the traces of physically similar items belonging to different arbitrary classes! Such neurological speculations are highly implausible. We conclude that similarity defined in terms of class membership has no relevance to physiological hypotheses derived from the principle of isomorphism. In using such a definition of similarity, Köhler failed to test his own hypothesis.

Color and size, which were used to define similarity in the fourth experiment, at first glance seem to be more

plausible as determinants of cortical interaction than is membership in an arbitrary meaning class. Size at least is a physical characteristic to which the hypothesis of (spatial) isomorphism is directly relevant. There is no clear evidence for the cortical localization of color (4). What type of spatial differentiation among traces is to be assumed when stimuli vary in both size and color, as they did in Köhler's experiment? There is no answer to this question based on physiological evidence, and Köhler's theory is inevitably silent on this point. At best, then, the results of the experiment on physical similarity may provide indirect support for a vague physiological hypothesis. Since this experiment is the only one that may have some relevance to the theory, its reproducibility assumes critical importance.

Köhler's analysis of association theory. Granted that Köhler largely failed to test his own hypothesis, what are the implications of his findings for association theory? The phrase "association theory" covers a multitude of theoretical positions, but Köhler focuses his criticism on two points which he apparently considers common to all associationistic approaches. First, the relationship between the stimulus and response items is a matter of indifference. Contiguity is the basic determinant of association. Second, Köhler suggests that association theorists agree on a *physiological theory* which is coordinate with the conception of association as an "indifferent bond." According to Köhler, association theorists identify learning with changes in conductivity along "individual nerve fibers" linking the central representations of S and R. In the light of his experimental findings, Köhler finds such a neurological theory grossly inadequate to account for the facts of learning.

This physiological theory of associa-

tion represents a straw man of the critic's own construction, or, at best, a confusion of outdated historical doctrine with current positions. It is true that Thorndike (e.g., 9) identified S-R connections with neural bonds, but such physiological speculations remained incidental to Thorndike's main theoretical effort and did not exert any direct influence on his experimental work. Beyond locating the mechanisms of association in the "life processes" of the neurons, Thorndike never offered detailed physiological explanations of specific experimental results.

In any event, contemporary association theory is certainly not built upon Thorndikian neurology; in fact, it is usually not built on any neurology at all.² The major constructs of modern learning theories, such as habit or expectancy, are symbolic constructs without any specific physiological connotation. Both Hull and Guthrie have been prone to describe stimulus effects and responses in "quasi-physiological language," but have no specific physiological theory of association. Other association theorists bypass the problem of physiological translation even more completely. There is no speculation concerning physiological processes in Tolman or Skinner. Indeed, Skinner explicitly rejects any recourse to neurological hypotheses as premature and misleading (8, p. 193 f.). Certainly "functionalists" such as McGeoch never veered from the path of strictly behavioral analysis (6). We do not know what association theorist was the target of Köhler's attack. The Thorndike of *Animal Intelligence* (1898)? The Thorndike of *Human Learning* (1931)?

²The work of Hebb (2) is an important exception. His theory emphasizes complex interactions in the nervous system, and bears little resemblance to the type of physiological hypotheses that Köhler chose to ascribe to associationists.

Hull? Tolman? Skinner? McGeoch? Guthrie? The criticism is either out of date or does not apply. We conclude that Köhler's experiments have no bearing on the physiological hypotheses of associationism. It remains for us to examine the results in relation to associationist behavior theory.

Perceptual similarity or generalization? Köhler suggests that there are no principles of association which can account for his experimental results. The difficulty arises, however, from Köhler's formulation of the problem rather than from an inadequacy of associationistic principles of learning. Köhler chose to analyze the learning of a *series* of paired associates with exclusive reference to the properties of *individual* pairs. He makes this point quite explicitly when he says, "... our problem refers to *individual* pairs and the degree of resemblance between their members" (3, p. 498). When the learning material consists of a set of items, any analysis which fails, as Köhler's does, to take account of the characteristics of the set will be necessarily incomplete. The critical importance of intralist relationships has long been recognized. Gibson (1) has systematized the role of intralist effects by an application of the principles of generalization and differentiation. Gibson's thesis is that the course of learning is a function of the degree of generalization among the items to be learned. Generalization is, in turn, a function of similarity. To the extent that generalization favors incorrect responses, i.e., when similar items require different responses, learning is delayed. To the extent that generalization favors correct responses, i.e., when similar items require the same response, learning is facilitated. It will be our contention that the advantage of homogeneous over heterogeneous pairs is a function of intralist generalization.

Let us turn first to Köhler's two ex-

periments in which *Ss* learned separate series of homogeneous and heterogeneous pairs composed of nonsense syllables, numbers, and geometric designs. We must consider not only the similarity between the two members of each pair but also the similarity among the cue items and among the response items. Both the cue items and the response items will be categorized by *S* as belonging to three classes: syllables, numbers, and designs. This grouping will be mediated by the differential responses which *S* is assumed to make to the individual items (5). These differential responses include labeling or identification of the items as members of one of the three classes. By virtue of a common differential response, items belonging to the same class will be more similar to each other than they will be to members of other classes.

Mediated stimulus generalization will occur primarily among similar items, i.e., items belonging to the same class. Thus, there will be more generalization between two syllable cues than there will be between a syllable cue and a number or design cue. When the pairs are homogeneous, similar cues will all require responses belonging to *one* class, e.g., all syllable cues will require syllable responses. Generalization will strengthen *S's* tendency always to give a response belonging to the appropriate class. He then merely needs to differentiate between the items *within* a class. By contrast, when the pairs are heterogeneous, similar cues require responses from *two* different classes. Generalization between cues will lead to responses from the incorrect class, e.g., responding to a syllable with a design when a number is correct, etc. In order to learn the series of heterogeneous pairs, *S* must eliminate two types of errors: (a) giving responses from the wrong class, and (b) confusion between the items within a given class. The *S* learning a homo-

geneous series has to overcome only the second type of error. Hence, homogeneous pairs will be learned faster than heterogeneous pairs. The effects of generalization may also be described in somewhat different terms. When the series is homogeneous, generalization tendencies conform to an unequivocal *rule of response selection* (syllables go with syllables, numbers with numbers and designs with designs). When the series is heterogeneous, the rule is equivocal. Responses may not come from the same class as the cue items, but they may come from one of two other classes. The more equivocal the rule, the more opportunity there is for intralist errors.

Our argument may be supplemented by a more formal analysis of the associative patterns established during the acquisition of homogeneous and heterogeneous lists. Refer to Figure 1 (left) which presents the assumed pattern of associations for Köhler's list of homogeneous pairs. A differential response (*r*) is linked with each cue item (*S*). The performance of this differential response results in response-produced stimulation (s). Differential responses and response-produced stimuli are more sim-

ilar for members of the same class of items than they are for members of different classes. Response items are similarly differentiated in terms of *r-s* patterns. In the course of learning, two types of associations are established: (a) "direct" associations between *S* and *R*, and (b) associations between *S* and *R* which are mediated by the differential response to cue items and response items, e.g., $S_{x_1} \rightarrow r_{x_1} \rightarrow s_{x_1} \rightarrow r_{x_2} \rightarrow s_{x_2} \rightarrow R_{x_2}$, where x_1 and x_2 are, say, two syllables forming a homogeneous pair. Generalized responses arise by virtue of similarities among the *Ss* as well as among the *ss*. The dashed arrows in Figure 1 indicate examples of generalized responses mediated by *r-s* sequences. Note also that the similarity between *r-s* sequences connected with *response items within a given class* will result in response generalization. It is clear that the generalized responses conform to a rule of response selection, and tend to restrict intralist errors to confusion of items within a class. Figure 1 (right) presents the pattern of associations for the heterogeneous list. Applying the same analysis as before, we find that generalization fails to follow an un-

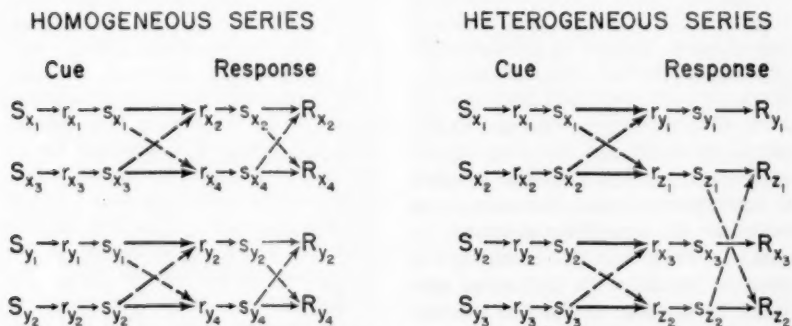


FIG. 1. Mediated associations formed during learning of homogeneous and heterogeneous lists. *S* = cue; *r* = differential response; *s* = response-produced stimulation resulting from *r*; *R* = overt verbal response. The subscripts, *x*, *y* and *z*, denote classes of items, i.e., syllables, numbers, and responses. Dashed arrows denote generalization tendencies. Only mediated associations are shown, but direct *S-R* connections are also assumed to be formed. To simplify exposition, only four of the six pairs in Köhler's series are represented.

equivocal rule of response selection and maximizes the opportunity for intralist errors, i.e., leads to errors both between classes and within classes.

Finally, we must consider the effects of transfer of training, i.e., the influence of the response dispositions with which *S* enters into the experimental situation. Köhler anticipates that association theorists would be inclined to argue that "syllables go with other syllables in speech, writing and reading, and that, similarly, numbers go with numbers in daily experience" (3, p. 499).³ We agree. Köhler dismisses the importance of this fact primarily because "previous associations of this kind will constitute vague reproduction tendencies in many different directions" (3, p. 499). Hence, there could be no positive transfer to the acquisition of *specific* homogeneous pairs; indeed, the transfer may be negative. Here we disagree. We suggest that *S*'s previous experiences with sequences of syllables, numbers, etc., will result in a significant bias for a *rule of response selection*, viz., the rule that like goes with like. In terms of the symbols of Figure 1, what will be significant is not the transfer of specific S-R linkages but rather linkages of the type, $s_x \rightarrow r_x$. There will be a generalized disposition to respond to syllables with syllables, to numbers with numbers, etc. When the series consists of homogeneous pairs, this disposition conforms to the rule of the series, and will favor correct performance. When the series consists of heterogeneous pairs, this disposition will delay the acquisition of correct responses. Thus, transfer of training will serve to intensify the differences produced by intralist effects and increase

the advantage of the homogeneous series.

Let us now consider Köhler's third experiment, in which homogeneous and heterogeneous pairs appeared in the *same* series. Only two types of items—syllables and designs—were used. There were four pairs: two homogeneous and two heterogeneous. The series does not obey a rule of response selection, i.e., intralist generalization does not favor the homogeneous pairs. The assumption of an initial response bias does, however, lead to the prediction that homogeneous pairs will be learned faster than heterogeneous pairs. In the case of homogeneous pairs, *S* can continue to apply the rule, and needs only to differentiate between the specific items in a given class. The heterogeneous pairs must, on the other hand, be learned in spite of the rule of response selection which *S* favors.

Experimental implications of the associationistic interpretation. So far, our reinterpretation of Köhler's results may be considered *ad hoc*. Fortunately, it is possible to decide between his interpretation and ours by experimental tests. We have asserted that in Köhler's experiments the operation of a rule of response selection favored homogeneous pairs over heterogeneous pairs. Köhler's interpretation hinges on the similarity between the members of *individual* pairs. To decide between these alternatives, it is necessary to compare the acquisition of a homogeneous and heterogeneous series when *both* obey a rule of response selection. This will be true when the series are composed of two rather than three classes of items. For the homogeneous series, the rule of response selection, then, is that like items always go together; for the heterogeneous series, unlike items always go together. In both cases the class membership of the stimulus fully predicts the class membership of the response. Ac-

³ Köhler also suggests that no transfer could be assumed in the case of geometric designs. The reason for this statement is not clear. Abstract designs may be less frequent than syllables and numbers but are apt to be encountered in groups or sequences.

According to Köhler's hypothesis, the difference in favor of homogeneous pairs should occur as clearly as in the earlier experiments. According to our hypothesis, the difference should be eliminated, except for the effects of initial response bias. The differential effects of initial bias should, however, be transitory, since the rule that like items go together would never lead to correct responses in the learning of the heterogeneous series. In addition, our analysis predicts, and Köhler's does not, that *Ss'* errors will reflect the rule of response selection. Our prediction follows directly from the generalization hypothesis. Köhler's interpretation does not provide for systematic variation in intralist errors as a function of the characteristics of the series, since his argument makes reference only to the composition of individual pairs. Our hypothesis implies, on the other hand, that the predominant type of error will vary directly with the rule of response selection.

In the case of a series composed of both homogeneous and heterogeneous pairs, the assumption of an initial response bias leads to the prediction that the homogeneous pairs will be learned faster. Here our prediction agrees with Köhler's. Again, however, our analysis can, and Köhler's cannot, generate a specific prediction concerning the distribution of errors. Since *S* has an initial disposition to link like items together, the errors at the beginning of practice should be predominantly in accordance with this rule. As training continues, and heterogeneous pairs begin to be learned, the relative frequency of such errors should decrease steadily until the total list has been learned.

Our reinterpretation of Köhler's results does not apply to his final experiment in which the physical resemblance of the members of the pairs was varied. As indicated earlier, the results of that experiment were much less clear-cut

than those of the other studies. We doubted the reproducibility of these results, and, as we shall see, our suspicions were confirmed.

Since our main concern is with the theoretical issues, we have refrained from a detailed criticism of Köhler's experimental method. Some of the most serious difficulties must, however, be mentioned briefly. (a) A fixed number of three presentations was used. Since difficulty varied from series to series, it is probable that the performance of the various groups was measured at different stages of learning. (b) For a given *S*, the order of presentation appears to have been constant from trial to trial. This is a highly unusual procedure for an investigation of paired-associate learning. A fixed order of presentation leads to serial connections among responses. It then becomes impossible to say to what extent performance depends on cue-response linkages, and to what extent on response-response linkages. (c) The analysis fails to take account of *Ss'* errors which offer important clues to the nature of the associations formed during training. The questions raised by Köhler's analysis can be answered adequately only by charting the systematic changes in *S*'s behavior throughout the course of learning to a criterion. In short, the experimental problem calls for use of the standard procedure of paired-associate learning (7, p. 15).

Our own experiments were, therefore, designed to answer the following questions under standard conditions of paired-associate learning: (a) Do series of homogeneous pairs and series of heterogeneous pairs differ in speed of acquisition when both series obey a rule of response selection? (b) Is there a difference in speed of acquisition between homogeneous and heterogeneous pairs within a single series? In both (a) and (b) the terms "homogeneous" and "heterogeneous" refer to the clas-

sification of items by meaning. (c) Are Köhler's findings concerning the effects of the physical resemblance of items within pairs reproducible? (d) What light is thrown on the relative merits of Köhler's theory and an associationistic interpretation by a detailed analysis of the course of acquisition and the distribution of the errors?

Experimental tests. We used the procedure customary in the investigation of paired-associate learning. The cue-response pairs were presented on a Hull-type memory drum. The cue item was presented for two seconds, followed immediately by a two-second exposure of the complete pair. There was an interval of eight seconds between trials.⁴ After an initial reading of the list, the anticipation method was used to a criterion of one perfect recitation. Four different orders of the pairs were used in rotation.

In Experiment I we compared learning of a homogeneous list and a heterogeneous one when both lists obey a rule of response selection. Therefore, only two types of items—syllables and numbers—were used. The pool of learning items consisted of eight nonsense syllables of 6.67 per cent to 40.00 per cent association value, and eight two-digit numbers. The homogeneous series consisted of four pairs of syllables and four pairs of numbers. The heterogeneous series consisted of four syllable-number pairs and four number-syllable pairs. Both the cue and response items were identical for the two series; the only difference between the homogeneous and heterogeneous lists was in the way the items were combined. Thirty-two Ss were used, 16 under each of the two conditions.

The following categories were used in

the analysis of Ss' performance: (a) correct anticipations, (b) failures to respond, and (c) overt errors. Overt errors included intralist intrusions, responses from outside the list, and incomplete responses. All overt errors were divided into two categories: *like errors* and *unlike errors*. A like error occurred when the incorrect response belonged to the same class of items as the cue, i.e., an incorrect syllable response to a syllable cue or an incorrect number response to a number cue. An unlike error occurred when the incorrect response belonged to the class of items different from the cue, i.e., an incorrect syllable response to a number cue or an incorrect number response to a syllable cue.

When the lists both conform to a rule of response selection, they are learned with equal speed. The mean number of trials to criterion was 23.88 for the homogeneous list, and 23.00 for the heterogeneous list. The slight superiority of the heterogeneous list (!) is, however, not significant. For *both* lists, Ss' responses conformed to the rule of response selection. Of a total of 807 overt errors made by Ss learning the homogeneous list, only three were unlike errors. Similarly, of the 578 errors made by Ss learning the heterogeneous list, only eight were like errors. The two rules of response selection had, however, different effects on the course of learning.

Figure 2 shows Vincent curves for correct responses, failures to respond, and overt errors under the two conditions.⁵ Under the homogeneous condition there are a larger number of correct responses *and* a larger number of

⁵ For purposes of constructing Vincent curves, the total number of trials for each S was divided into five equal parts. The number of correct responses, failures, and overt errors per trial was then determined for each fifth. The means of these values are plotted in the figure.

⁴ Köhler does not report the temporal intervals used in his experiments, so that we do not know whether our intervals are comparable to his.

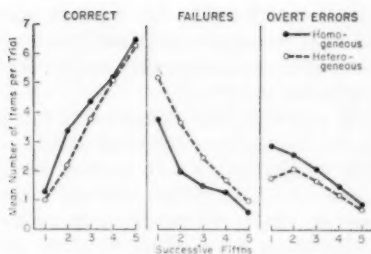


FIG. 2. Vincent curves for correct responses, failures to respond and overt errors in Experiment I.

overt errors than under the heterogeneous condition. Consequently, there are also fewer failures to respond under the homogeneous condition. For each response classification, the mean difference between the homogeneous and heterogeneous condition is significant at either the .05 or the .01 level. The two groups necessarily converge in all respects as the criterion of perfect recitation is approached.

If we were to measure correct responses at some arbitrary point early in learning, we would appear to obtain results similar to Köhler's, i.e., better recall for the homogeneous pairs. It is now clear that to ascribe these results to the facilitating effect of perceptual organization on recall would be a mistake, since Köhler's analysis cannot account for the difference in errors between the two conditions. The higher frequency of both correct and incorrect responses under the homogeneous condition readily falls into place on the assumption that Ss initially favor the rule of response selection appropriate to that condition.

Experiment II compares the learning of homogeneous and heterogeneous pairs within a single series. The same cue and response items were used as in Experiment I, except that the items were so arranged as to form four homogeneous and four heterogeneous pairs. Half the

homogeneous pairs consisted of syllables, the other half consisted of numbers. Similarly, half the heterogeneous pairs consisted of syllable-number combinations, and the other half of number-syllable combinations. Sixteen Ss learned the list under the same conditions as used in Experiment I.

The mean number of trials to criterion was 23.81. The over-all speed of learning was, therefore, almost identical with that in Experiment I. Homogeneous pairs were, however, learned much more rapidly than heterogeneous pairs. The mean number of trials required for correct anticipation of all homogeneous pairs in a single trial was 15.75. In the case of heterogeneous pairs, this criterion was reached after 22.81 trials. This difference is significant ($p < .01$). Note that the number of trials to criterion for homogeneous and heterogeneous pairs is virtually identical when the two types of items appear in separate lists. When both appear in the same list, homogeneous pairs have a striking advantage.

We have predicted speedier learning for homogeneous pairs on the assumption of an initial bias in favor of like responses. The existence of such a bias is clearly demonstrated in Table 1, which lists the mean number of like and unlike errors per trial for successive

TABLE 1
AVERAGE NUMBER OF LIKE AND UNLIKE ERRORS
PER TRIAL FOR SUCCESSIVE FIFTHS OF
LEARNING PERIOD IN EXPERIMENT II

Successive Fifths	Homogeneous Pairs		Heterogeneous Pairs	
	Like Errors	Unlike Errors	Like Errors	Unlike Errors
1	.90	.27	.78	.46
2	.71	.18	1.06	.48
3	.43	.10	.72	.28
4	.32	.12	.54	.58
5	.19	.07	.30	.25

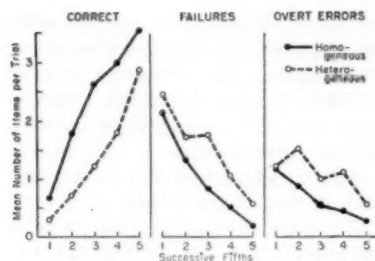


FIG. 3. Vincent curves for correct responses, failures to respond and overt errors in Experiment II.

fifths of the learning period. Like errors predominate, especially in the early stages of learning. While like errors decline steadily over successive fifths, unlike errors show only irregular variations. Progress toward the criterion is signalled primarily by the elimination of the initially frequent like errors. Both the mean difference between like and unlike errors and the interaction between type of error and stage of learning are significant ($p < .01$). We should add that, throughout the course of learning, both types of errors are significantly more frequent for the heterogeneous pairs than for the homogeneous pairs.

Figure 3 shows Vincent curves for the different classes of responses in Experiment II. As in Experiment I, we find a larger number of correct responses for homogeneous pairs. In contrast with Experiment I, we now find a greater frequency of overt errors as well as failures for heterogeneous pairs. For each response classification, the mean difference between homogeneous and heterogeneous pairs is significant ($p < .01$). As noted above, like errors predominate for both types of pairs. Such errors continue to be given to the heterogeneous pairs even after the homogeneous pairs have reached a high level of learning. In short, the response bias

favors the learning of homogeneous pairs but produces a sustained high level of errors and slow learning for the heterogeneous pairs.

As predicted, the difference in favor of the homogeneous pairs is considerably enhanced when both types of items appear in the same list. The difference between the two experiments is easily explained on the assumption of an initial response bias. Köhler's analysis could not have predicted the difference between Experiments I and II. In fact, his method of measurement—determination of correct responses regardless of errors after an arbitrary number of trials—revealed no comparable differences in his own study.

In Experiment III we attempted to reproduce Köhler's finding that pairs homogeneous in size and color are learned faster than pairs which are heterogeneous in these respects. The list of cue-response pairs and the conditions of practice were exactly the same as in Experiment II. For one group of 16 Ss, the cue and response items were homogeneous in size and color. For half the group, all items were large ($\frac{5}{16}$ in. high) and black; for the other half, all items were small ($\frac{3}{16}$ in. high) and red. For another group of 16 Ss, the cue and response items were heterogeneous in size and color. For half the group, the cue items were large and black, and the response items were small and red; for the other half, the cue items were small and red and the response items were large and black. In the present experiment, therefore, homogeneity and heterogeneity refer to the size and color of the items rather than to their membership in conventional meaning classes.

The mean number of trials to criterion was 26.75 for the homogeneous group, and 23.56 for the heterogeneous group. The difference in favor of the heterogeneous (1) group was not significant. Since Ss were not required to

recall the size and color of the response items, an analysis in terms of like and unlike errors is impossible. Figure 4 shows Vincent curves for the homogeneous and heterogeneous conditions. The slight differences between the two groups in the number of correct responses and overt errors do not approach statistical significance. The mean number of failures per trial is, however, significantly smaller ($p < .05$) for the heterogeneous condition. We have no adequate explanation for the slight superiority of the heterogeneous condition. One possibility which we have considered is that the heterogeneous condition reduced Ss' tendency to reproduce cue items instead of response items. The frequency of such cue intrusions was small and variable. For what it is worth, a count of the cue intrusions shows some greater frequency for the homogeneous condition, (55), than for the heterogeneous condition, (32).

Thus, the results not only fail to support Köhler's findings but tend to point in the opposite direction. The type of response bias which allowed us to account for the results of Experiments I and II could not operate in this situation, since S's responses were independent of the properties of color and size. The failure to find a difference in favor of the homogeneous pairs is, therefore, consistent with our interpretation of the

earlier experiments. It is not consistent with Köhler's perceptual hypothesis.

Our experimental results fail to provide any support for Köhler's hypothesis. It is possible to account for all the findings in terms of principles of associative learning. These facts do not in themselves constitute a disproof of Köhler's position. His hypothesis remains an hypothesis for which there is as yet no experimental evidence.⁶

The weakness of perceptual analogies in the analysis of memory. The difference between Köhler's analysis and ours again brings to the fore an issue which has long divided Gestalt theorists and associationists: the relation between perception and memory. The Gestalt theorist is concerned with the effects of perception on memory. Primary perceptual experience leaves a pattern of traces; the arousal of these traces mediates recall. In the analysis of learning and retention the Gestalt theorist is thus forced to maintain a precarious distinction between an initial experience (perception) and its later consequences (memory). It is difficult to specify where one ends and the other begins. Like the spurious present, the exact moment at which a primary perception changes into a memory or a perception-plus-memory must, we suspect, elude even the most sophisticated phenomenologist. Yet, the Gestaltist insists on looking for the influences of primary perceptions on memory. In his preoccupation with the persisting effects of phenomenal experience, he pays little attention, if any, to the

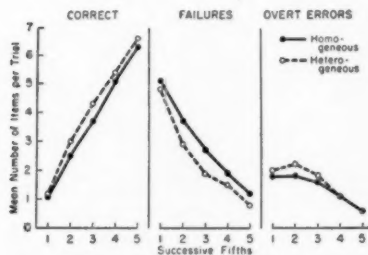


FIG. 4. Vincent curves for correct responses, failures to respond, and overt errors in Experiment III.

⁶We have not considered an experiment briefly mentioned by Köhler attempting to show that cues and responses that "fit together" are learned better than those that do not. We have not felt it possible to deal with this problem, since the obvious difficulties involved in an independent definition of "fittingness," as distinguished from familiarity, have not been resolved. Moreover, only a very sketchy account of the procedures and results is given.

conditions which influence *S's responses* from which inferences about both perception and memory must be made. Köhler's failure to consider the errors as well as the correct responses in relation to the characteristics of the series is an excellent case in point. Stimuli, "experiences," and "traces" cannot tell the whole story. There are always the responses that we must worry about.

The association theorist does not draw a sharp conceptual distinction between the initial "perception" and the recall tested at a later time. Rather, he is concerned with the *changing probabilities of different stimulus-response sequences* as a function of training. In paired-associate learning, for example, the correct response can be given initially only when the response item is exposed in the window of the memory drum ("perception"). Gradually, the cue items come to elicit the correct responses before the total pair has been exposed ("memory"). But the cue items also elicit incorrect responses ("memory?"). The changes in the probabilities of different responses vary systematically with the characteristics of the series and the conditions of practice.

The difference between this formulation and that of the Gestaltist is not purely semantic. The emphasis on con-

ditions of responding (a) avoids the methodological impasse of distinguishing between primary perceptions and memories, and (b) avoids a one-sided emphasis on sensory input at the expense of a careful analysis of the learner's behavior.

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